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TRANSACTIONS

OF THE

ROYAL ACADEMY OF MEDICINE IN IRELAND.

TRANSACTIONS

OF THE

Royal Academy of Medicine

IN IRELAND.

VOL. XIV.

EDITED BY

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RULES.

1. The name shall be, "ROYAL ACADEMY OF MEDICINE IN IRELAND." (1887)

Constitution.

2. The Academy shall consist of Fellows, Honorary Fellows, Members, and Student Associates.

Management.

3. The affairs shall be managed by a Council, consisting of the President, Ex-Presidents (1893), the six Presidents of Sections, the General Secretary and Treasurer, the Secretary for Foreign Correspondence, six Secretaries of Sections, and eight Councillors, being two representatives from the Medical, Surgical, Obstetrical, and Pathological Sectional Councils respectively.

Meetings.

4. The Meetings shall be General and Ordinary.

Publication of "Transactions."

5. The "Transactions" shall be published by the Council, subject to the provisions hereinafter contained.

Original Fellows and Members.

6. All the Members of the present Societies (Medical, Surgical, Obstetrical, and Pathological) shall be Original Fellows or Members, without entrance fee, on payment of the annual subscription on or before 31st December, 1882.[a]

Fellows.

7. Fellows of the King and Queen's College of Physicians in Ireland, and of the Royal College of Surgeons in Ireland, shall be admitted, without ballot, on payment of the entrance fee and the subscription for the current year. All others, being Registered Medical Practitioners not directly or indirectly engaged in the sale of drugs, shall be proposed by two Fellows, and elected by ballot by the Council.

8. Candidates shall be proposed at one Meeting of the Council, and balloted for at the next—one black bean in four to reject.

8A. That all Rules referring to the admission of Fellows, Members, and Student Associates shall be interpreted as referring to Ladies as well as Gentlemen.

Privileges of Fellows.

9. Fellows only shall be eligible for office in the Academy. They shall have the privilege of attending all Meetings of the Academy, of making Communications, and of voting and speaking at such meetings. They shall also receive a copy of the "Transactions."

[a] Those who have paid a Life Subscription to any of the above Societies will be admitted to the privilege of Fellows on payment of Member's subscription.

10. These privileges shall not be exercised by any Fellow in arrear with his subscription.

Honorary Fellows.

11. Honorary Fellows, limited in number to 25, may be nominated by the Council, and elected, on motion at a General Meeting of the Academy by a majority of at least two-thirds of those present and voting.

Members.

12. Any Registered Medical Practitioner may be elected as a Member, the election to be conducted in the same manner as that of Fellows.

Privileges of Members.

13. Members shall have the privilege of attending the Ordinary Meetings of the Academy, of making Communications, and of taking part in debate. They can purchase the "Transactions" at cost price.

Student Associates.

14. Registered Medical Students, of the third or subsequent years, may be elected as Student Associates in the same manner as the Members.

15. Student Associates shall have the privilege of attending the Ordinary Meetings of the Academy.

Annual Subscription.

16. Fellows shall pay £2 2s., and Members £1 1s. Student Associates shall pay 5s. The Subscription shall become due on the 1st of October in each year, and if the Subscription be not paid on or before the first Meeting in February, the defaulter shall cease to belong to the Academy, unless the delay shall be accounted for to the satisfaction of the Council. No Fellow shall vote at the Annual General Meeting who has not paid his subscription for the year. Medical Officers of the Army and Navy, and Registered Medical Practitioners not residing within 15 miles of Dublin, are eligible as Fellows of the Academy on payment of the entrance fee, and an annual Subscription of £1 1s.

Entrance Fee.

17. After admission of Original Fellows, all Fellows shall pay an entrance fee of £1 1s.

Council.

18. The Council shall meet on the first Wednesday in the month throughout the Session, or oftener should they see occasion—five to form a quorum.

19. Notice of all Extraordinary Meetings shall be transmitted by the Secretary to every Member of the Council. The President or any five Members of Council may call an Extraordinary Meeting of the Council. The Council shall determine questions by vote, or by division if so demanded, the President having a casting vote only. Any regulation of the Council shall have the force of a law, until submitted to the next General Meeting. The Council shall have the power of filling up any vacancies which may occur in the list of Officers of the Academy, except that of President, before the Annual General Meeting. If a vacancy in the office of President should occur, the General Council shall summon a Special General Meeting of the Academy to fill such vacancy. (1888.)

Sectional Councils.

20. There shall be six Sectional Councils elected by the Annual General Meeting in October, termed respectively—the Medical, the Surgical, the Obstetrical, and the Pathological, the State Medicine, and the Anatomical and Physiological Councils.

21. No Fellow shall be eligible as a candidate for election on more than two Sectional Councils, but no Fellow shall be eligible as a candidate for election on both the Medical and Surgical Sectional Councils. (1888.)

22. Each Sectional Council shall consist of the President of the Section and ten Members, one of whom shall act as Secretary to the Section; except the State Medicine and Anatomical and Physiological Councils, which shall each consist of a President and six Members. (1888).

Meetings of Sectional Councils.

23. Each Sectional Council shall meet on a fixed day at least one week before the Ordinary Meeting of their Section, three to form a quorum.

Powers.

24. Each Sectional Council shall have the power of making any such arrangements as it thinks necessary to carry on the work of the Ordinary Meetings which are under its charge, provided that such arrangements do not interfere with the general laws of the Academy; and any Rules laid down by such Council shall have the force of laws at the Ordinary Meetings under its charge, until submitted to the General Council.

25. Each Sectional Council shall have the power of filling up any vacancies that may occur among its Members until the Annual General Meeting.

Committee of Reference.

26. The Council shall appoint a Committee of Reference, to report upon morbid growths and other specimens exhibited before the Academy; of this Committee the Exhibitor shall, for the occasion, be a Member.

Officers.

27. A President, to be elected by the Annual General Meeting in October, and to hold office for three years.

28. The Presidents of the Colleges of Physicians and Surgeons for the time being shall be the Presidents of the Medical and Surgical Sections. The Presidents of the other Sections shall be elected by the Fellows at the Annual General Meeting, and shall hold office for two years. (1888.)

29. One General Secretary and Treasurer to be elected at the Annual General Meeting.

30. It is expedient that a fixed salary (of one hundred guineas) shall be paid yearly to the General Secretary in consideration of the fact that the editing of the "Transactions" is part of his duties.

31. One Honorary Secretary for Foreign Correspondence to be elected at the Annual General Meeting. (1888.)

32. The Councillors for each Section to be elected at the Annual General Meeting. Each Sectional Council shall elect two Members to act on the

General Council, except in the case of the Sections of State Medicine and Anatomy and Physiology. (1888.)

33. Two Members in each Sectional Council shall retire annually, and be ineligible for re-election for one year, except in the Council of the Section of Anatomy and Physiology, in which only one shall retire (Oct., 1896).

34. Six Secretaries, one for each Section, to be appointed by the Sectional Councils.

35. At all elections after the year 1882, any Fellow desirous of nominating a candidate for election shall, at least one fortnight before the Annual General Meeting, forward an application to the General Secretary to enter the name of such Fellow on the list of candidates for office, provided that the Fellow so nominated shall have consented to act. (1891.)

36. That all elections shall be by ballot, but Fellows residing more than 15 miles from Dublin, and those incapacitated from attending by illness (to be certified), may record their votes by ballot papers, sent to the presiding officer in sealed envelopes provided for that purpose (Oct. 1896).

Duties of Officers.

37. *The President* shall preside at the Annual and Special General Meetings and at General Council Meetings. In the absence of the President, the Chairman shall be appointed by the meeting. (1888.)

38. *The Presidents of Sections* shall preside at the Ordinary Meetings of the Academy, and shall also preside at the Sectional Council Meetings. In the absence of the President, the Chairman shall be appointed by the Meeting. (1888).

39. *The General Secretary* shall attend all General Meetings of the Academy and General Council. He shall take minutes of such meetings, to be read at the following meeting.

40. He shall receive and have charge of all papers intended for publication in the "Transactions" of the Academy, after they have been handed over to him by the Secretaries of the several Sections.

41. He shall, on receiving notice from the Secretary of a Section, send out to all the Members notices of the title or titles of the paper or papers for the next Ordinary Meeting, with the name or names of the authors, and, so far as possible, of the subjects for Exhibition, with the names of the Exhibitors.

42. He shall arrange for the Exhibition of specimens and the reading of papers, which are forwarded to the Academy by those who are absent, or are not members.

43. The General Secretary and Treasurer shall receive all moneys, and lodge the same in bank to the account of the Academy, and all cheques shall be signed by the Treasurer and one other Councillor.

44. The Accounts shall be audited by two Fellows, not Members of Council, to be appointed by the President at some meeting previous to the Annual Meeting.

Duties of Secretaries of Sections.

45. To attend the Meetings of the Council of the Section and the Ordinary Meetings of the Academy, under the management of said Council, and to take minutes at such meetings, to be read at the next following meeting of that Section.

46. To keep such papers as the Sectional Councils deem worthy of publication, for the purpose of handing them over to the General Secretary.

47. To inform the Secretary of the Committee of Reference of any specimens referred to that Committee, and to transfer the specimens to that Secretary.

48. To give notice to the General Secretary, one week previously to the meeting, of the titles of papers for the evening, the names of the authors, and, so far as possible, the objects for Exhibition, with the names of Exhibitors, so that the General Secretary may inform the Members.

Meetings.

49. The Annual General Meeting to take place on the last Friday in October, for the election of Officers and Members of Council, and for the general business of the Academy.

50. Due notice of the meeting shall be given by the Secretary to all members at least three weeks previously. (1891.)

51. No motion involving a change of these Rules shall be brought before this meeting except one week's notice thereof shall have been given by the Secretary to each Member.

52. The President may—and shall forthwith, on receiving a requisition signed by seven Fellows, at any time—on giving one week's notice, summon a Special General Meeting, for the consideration of particular business, the nature of which must be specified in the letter of summons convening the meeting, and at such meeting no other business can be transacted. In the event of the President being unable, from any cause, or declining, to summon a Special General Meeting of the Academy, it shall be in the power of the General Council to summon such meeting. (1888.)

Ordinary Meetings.

53. The communications to be submitted to the Ordinary Meetings shall be grouped under the following heads :—Medicine, Surgery, Pathology, Obstetrics, State Medicine, and Anatomy and Physiology ; and the conduct of such meetings shall be in the hands of the several Sectional Councils, each Sectional Council to have the management of the Ordinary Meeting in rotation, as arranged by the General Council. (1888.)

54. The Ordinary Meetings shall be held on every Friday evening, from the first Friday in November until the last Friday in May, inclusive, at eight o'clock, except during the Christmas and Easter recesses.

55. All Fellows, Members, and Student Associates attending the meetings, shall write their names in the attendance book.

56. Any Fellow or Member may introduce two Visitors by cards obtained from the Sectional Secretaries.

57. Officers of the Army or Navy Medical Departments shall, on presenting their cards, be admitted to the Ordinary Meetings of the Academy.

58. No communication shall exceed twenty minutes in its delivery, nor any speech thereon ten minutes, except by permission of the Chairman. No one shall speak twice upon the same communication, except the author, who has the right of reply.

58 (*a*). A paper by any other than a Fellow or Member of the Academy shall not be read before the Academy unless the author of such a communication shall have obtained permission to do so from the Council of the Section before which the communication is proposed to be read. (1892.)

Ordinary Meetings.—Order of Business.

59. (1.) Chair to be taken at 8 30 p.m.

(2.) Chairman to read list of specimens, &c., exhibited by card, together with the names of the Exhibitors.

(3.) No Pathological Specimen shall be exhibited at any Section other than the Pathological and Obstetrical, except by card. This Exhibition shall not exclude any subsequent communication regarding it at the Pathological Section.

(4.) There shall be no Exhibition of Specimens by card in the Obstetrical or Pathological Sections.

(5.) Any member shall have liberty to exhibit any recent specimen at any of the meetings of the Obstetrical Section, provided it illustrates any question in gynæcology.

(6.) At the meetings of the Obstetrical Section recent specimens may be exhibited, and the President shall invite discussion thereon, provided that such exhibition of specimens or discussion, if any, thereon, must terminate at 9 o'clock, p.m., but that, if necessary, they may be resumed after the papers for the evening have been read and discussed.

(7.) Chairman to ask if any member has any observations to make or motion to propose relative to any living specimen on the List of Exhibition.

(8.) Chairman to call upon the author of the first paper on the list to read his paper.

(9.) Chairman to call upon members to discuss the paper, or, at his discretion, to take any other paper or papers on the list relating to the subject, and have the discussion subsequently on all such papers collectively.

(10.) When the last paper has been discussed, the Chairman to ask if any member desires to speak upon any of the specimens exhibited by card.

(11.) After the discussion upon any specimen, the Exhibitor has the right of reply.

Regulations regarding the Exhibition of Specimens by Card.

60. (1.) Any member may exhibit by card at any Ordinary Meeting, except at the meeting of the Pathological and Obstetrical Sections. At the meetings of the Pathological all specimens must be presented and described *viva voce*, and debate may be invited thereon.

(2.) Notice shall, if possible, be given to the General Secretary, or the Secretary of the Section, on or before the previous Ordinary Meeting.

(3.) Specimens must be in the room at 7 45 on the night of Exhibition.

(4.) Specimens for Exhibition by card shall be open for inspection at 8 p.m.

(5.) A card, containing all particulars for publication, shall be placed with the Specimen. Cards for this purpose are to be obtained from the Secretary.

(6.) The Exhibitor should be present, and he shall furnish further details if asked for.

(7.) Every Exhibitor shall submit the Specimen or Specimens on view to the Committee of Reference, if the meeting so decide.

Exhibition of Pathological Specimens.

61. No lengthened reference to treatment shall be allowed upon any Specimen, except by the express permission of the Chairman. Whenever it has been agreed that a Specimen exhibited at a Sectional Meeting of the Royal Academy of Medicine in Ireland shall be sent to the Reference Committee to report thereon as to its nature, the Exhibitor is to retain the custody of the specimen until he shall be summoned to a meeting of said Committee to be convened by its Secretary, on an early day, when he will attend and submit it for examination. (1889).

By-laws concerning "Transactions."

62. The "Transactions" shall consist of such Communications made to the Academy by or through Fellows or Members as may be deemed by the General Council suitable for publication; also, of discussions of importance or interest arising out of such Communications.

63. All Communications accepted by the Academy become the property of the Academy, but authors may also print their Communications, subsequent to the reading of the same before the Academy, in any publication in addition to the "Transactions." Papers shall be handed to the Secretary of the Section immediately after they have been read. (1891.)

64. The "Transactions" for the year shall be presented to all Fellows of the Academy who have paid their Annual Subscriptions.

65. The "Transactions" may be purchased by Members at cost price.

66. The Publication Committee of each Section shall meet not later than the Tuesday after each meeting of the Section, for the purpose of abstracting the proceedings—the abstract to be placed in the printer's hands on same evening, and forwarded to the editors of medical journals with the least possible delay. (1888.)

67. Contributors of papers are requested to send their papers to the Academy printer early enough to allow of their being put in type before the meeting, and read in proof. (1888)

68. That on the evening of the day of meeting of the Sectional Council, when the papers for the next meeting have been decided upon, a circular be sent to each contributor informing him :—

(1.) That he is expected to be ready or else take his place at the bottom of the list.

(2.) That he must have an abstract ready with his paper, otherwise he will be noted in the published proceedings in such form as the Publication Committee think fit.

69. The General Council is empowered to defray the expenses in whole or in part of any illustrations which it may consider advantageous to the elucidation of the papers published by the Academy.

70. An abstract (prepared by the author) of each communication made at the Academy, along with a report of the discussions thereon, shall be furnished to the editors of such medical journals as may desire to publish them, and the authors of such communications shall be empowered to publish their papers *in extenso* in any periodical or periodicals they may think fit, such communications also to appear in the "Transactions," provided the Council consider them worthy of insertion.

Expulsion of Fellow or Member.

71. Expulsion of a Fellow or Member can take place only at a General Meeting of the Academy, on the motion of the Council, if two-thirds of the Members present shall vote for the same by ballot. Of such ballot the Council must give at least fourteen days' notice in writing to every Fellow of the Academy.

New Laws.

72. New Laws, or alterations in existing Laws, can be proposed only at the Annual General Meeting. Any Fellow proposing such alteration shall give notice to the General Secretary at least ten days before the General Meeting in October.

REPORT.

THE General Council begs to report that the number of Fellows for the Session 1894-95 was 235; of Members, 31; and of Student Associates, 2. The Fellows decreased by 9, the Members by 1, and the Students by 2.

At the Annual Meeting last year a resolution was carried appointing a Sub-Committee to consider the best use to which the surplus funds of the Academy could be applied, with special reference to the purchase of Medical Literature, and it was ordered that the Committee should report to the Council.

The Committee met immediately, and had before it the lists of publications subscribed for by the Royal College of Surgeons and the Royal College of Physicians; and the Committee declared its opinion that a sum of £20 might be devoted annually to the purchase of periodicals to be entrusted to the keeping of the Colleges for the Academy. It was then decided to obtain from the Secretaries of the various Sections lists of the publications which the respective Councils considered it advisable to procure.

The Committee recommended that a sum of £20 should be expended—£15 on books and £5 on binding—half of these books to be sent to each College, with a request that they would keep them for the Academy.

A resolution was at the same time submitted from the Council of the Section of State Medicine declaring that a better method of disposing of the surplus funds would be to reduce the subscription for Fellows of a certain standing.

After prolonged discussions the General Council decided

that it is inadvisable to expend the funds of the Academy in the purchase of books.

A proposal was made that a demonstration lantern should be provided for use at the Sectional meetings; and the Council believing this to be a necessity, purchased the lantern and necessary apparatus at a cost of £20 9s. 9d.

The Council has not been able this year to increase the investments. This has been caused by the expenditure just mentioned, and the increased cost of illustrations for the last volume; but the Council is satisfied that the Academy will approve of what has been done in this regard.

JAMES LITTLE, *President.*
WILLIAM THOMSON, *General Secretary.*

General Treasurer's Report for the Session 1894–95.

RECEIPTS.

	£	s.	d.
To Balance in Bank	2	15	5
„ Subscriptions	483	10	0
„ Fannin, Illustrations	2	11	4
„ Dividend on £596 2s. 4d., 2½ per Cent.	15	17	1
„ „ £155 1s. 10d., 2½ per Cent.	3	17	8
Total	£508	11	6

EXPENDITURE.

	£	s.	d.
General Secretary	105	0	0
Grant to Royal College of Physicians	15	15	0
Grant to Royal College of Surgeons	15	15	0
Printing, '93-4	19	0	0
Fannin & Co., Vol. XII.	146	4	10
Illustrations,	84	11	6
Reporters	29	8	0
Stationery	2	13	9
Falconer, Printing	14	11	0
Servants	19	13	0
Lantern, &c.	20	9	9
General Advertiser, Printing and Postage	53	8	11
Sundries	4	1	4
Groceries	17	5	7
Report on Specimen	2	2	0
Milk	2	16	8
Balance in Bank	5	15	2
Total	£508	11	6

We have examined the Accounts and Vouchers, and find the same to be correct.

{G. P. L. NUGENT, M.D.
{ROBERT H. WOODS, F.R.C.S.

October 12th, 1895.

WM. THOMSON, *General Secretary and Treasurer.*

Volume XIV. of the "Transactions" has been forwarded to the following:—

Lancet	London.
British Medical Journal	Do.
Medical Press	Dublin.
Hospital Gazette	London.
Dublin Medical Journal	Dublin.
London Medical Recorder	London.
Edinburgh Medical Journal	Edinburgh.
Glasgow Medical Journal	Glasgow.
Liverpool Medical Journal	Liverpool.
Bristol Medical Journal	Bristol.
Asclepiad	London.
International Medical Journal	Do.
Annals of Surgery	Do.
Provincial Medical Journal	Leicester.
Birmingham Medical Review	Birmingham.
Sanitary Record	London.
Practitioner	Do.
College of Surgeons	Dublin.
Do.	London.
Do.	Glasgow.
Trinity College	Dublin.
Royal University	Do.
Queen's College	Belfast.
Do.	Galway
Do.	Cork.
Royal Dublin Society	Dublin.
College of Physicians	Do.
Do.	Edinburgh.
Do.	London.
Faculty of Physicians and Surgeons	Glasgow.
Victoria University	Manchester.
University College	London.
Harveian Society	Do.
Pathological Society	Do.
Clinical Society	Do.
Medical and Chirurgical Society	Do.
Apothecaries' Hall	Do.
King's College	Do.
University	London.
Do.	Glasgow.
Do.	Edinburgh.
Do.	Durham.
Do.	St. Andrews.
Do.	Aberdeen.
University College	Dundee.
Do.	Melbourne.
Do.	Adelaide.
Do.	Calcutta.
Do.	Bombay.

University College - - - - St. Petersburg
Do. - - - - - Paris.
Do. - - - - - Vienna.
Do. - - - - - Berlin.
Do. - - - - - Brussels.
Do. - - - - - Bologna.
Do. - - - - - Madrid.
Do. - - - - - Amsterdam.
Do. - - - - - Christiania.
Do. - - - - - Stockholm.
Do. - - - - - Toronto.
Do. - - - - - Quebec.
Do. - - - - - New York.
Do. - - - - - Philadelphia.
Do. - - - - - New Zealand.
Do. Library - - - - Tokio, Japan.
Medical Institute - - - - Birmingham.
Dr. Ashby - - - - - Do.

Director-General Billings, Washington, U.S.
Journal de Médecine et de Chirurgie (M. Lucas, Championière), Paris.
Archives de Chirurgie, 108 Boulevard St. Germain, Paris.
Archiv für Klinische Chirurgie, Berlin.
Centralblatt für die medicinischen Wissenschaften, Berlin.
Zeitschrift für Chirurgie, Berlin.
The Australian Medical Gazette, Sydney, N.S.W.
New York Medical Journal, 72 Fifth Avenue.
Journal, American Medical Association, Chicago.
Bulletin de l'Académie Royale de Médecine de Belgique, Brussels.
Medical Journal, Brooklyn.
Naturforschende Gesellschaft, Dr. Rudolf Martin, Seefeldstrasse 119, Zurich.
Academy of Medicine, 17 West 43d Street, New York.
Library, College of Surgeons, Edinburgh.
„ British Medical Journal, 429 Strand, London.
„ Medico Chirurgical Society, Bristol.
Sheffield Medical Journal, 17 Eyre Street, Sheffield.
Archives Cliniques de Bourdeaux, 46 Cours du Jardin-public, Bordeaux
Medical Society of Victoria, Melbourne (Meyer and Metzler, Great Portland, Street, London).
Quarterly Medical Journal, Dr. Coching, 277 Glossop-road, Sheffield.
Medical Library Association, 19th and Stout Street, Denver, Colorado, U.S.
Charlotte Medical Journal, Charlotte, N. C., United States.
Society of Medicine, care of Dr. J. V. Wichman, Copenhagen.
University of Sydney, care of Young J. Pentland, 38 West Smithfield, London, E.C.
Editor, Archivio d'Ortopedia, via S. Calimbro 31, Milan.
Library, Medical Society, Royal University, Upsala.
National Library, Dublin.
Journal of Comparative Neurology, Denison University, Granville, Ohio, U.S.
Dr Stockwell, Medical Age, Detroit, Michigan, U.S.
Archivio di Ortopedia (Dr. Pietro Panzeri, Instituto dei Rachitici, Milan).
Journal of Comparative Neurology, Dr. C. L. Herrich, Granville, Ohio, U.S.A.

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TRANSACTIONS
OF THE
ROYAL ACADEMY OF MEDICINE IN IRELAND.

SECTION OF MEDICINE.

THE INFECTIOUS HOSPITALS (DUBLIN) SCHEME.

By J. W. MOORE, M.D. Univ. Dubl., F.R.C.P.;
Ex-Scholar and Diplomate in State Medicine, Trin. Coll. Dubl.

[Read at a meeting of the combined Sections of Medicine and State Medicine, November 15, 1895.]

In introducing to the notice of the Royal Academy of Medicine in Ireland the draft Scheme, prepared by the Superintendent Medical Officer of Health of the City of Dublin, "for the formation of a Board for establishing Hospitals for Infective Diseases in the County Dublin," I may be allowed to express my surprise and regret that, before launching his scheme and submitting it to the public, Sir Charles A. Cameron did not think it advisable—not to say expedient—to take his own profession more fully into his confidence in a matter which involved so many professional as well as public interests.

It has been the custom for the State to consult the Royal College of Physicians of Ireland and the Royal College of Surgeons in Ireland whenever any grave topic affecting the Public Health has been under consideration. Surely,

then, the Superintendent Medical Officer of Health might well have sought the opinion of the Royal Colleges upon a hospital scheme which, if carried out, was bound to have a far-reaching influence upon Medical Education in Dublin, no matter what its effect upon the Public Health might be.

Sir Charles Cameron has rightly or wrongly decided otherwise, and therefore he cannot, and should not, complain, if his scheme is fated to be severely handled in the searching criticism to which it is bound to be exposed.

Perhaps it will be most convenient in the first place to point out the salient features of the "scheme."

1. An Act of Parliament is to be obtained to establish and incorporate a Board of Governors—40 in number—having control over "hospitals for small-pox and other infectious diseases," the area under the jurisdiction of the Board being practically the County of Dublin (including the City itself).
2. "The Board shall consist of Governors, nominated as follows:—8 by the Corporation; 10 by the South Dublin Union; 8 by the North Dublin Union; 2 by the Rathdown Union; 2 each by the Rathmines, Pembroke, and Kingstown Commissioners; and 1 each by the Blackrock, Dalkey, Killiney, Drumcondra, Clontarf, and Kilmainham Commissioners."
3. All the ordinary expenses of the Board are to be provided out of the Poor's Rate; but the Board "shall be empowered to borrow money for the purpose of procuring a site or sites, erecting and equipping hospitals and convalescent houses for infectious cases." "The cost of procuring sites, the erection and furnishing of buildings, the maintenance of medical and nursing staffs, shall be charged *pro ratâ* to each of the electoral divisions

of the three Unions" co-operating in the scheme, that is, "the South and North Dublin Unions, and so much of Rathdown Union as is situated within the County of Dublin."

4. The appointment of all officials—including Medical Officers—shall vest in the Board.
5. The exercise of all the powers possessed by the Board, except that of contracting for the erection of buildings or for supplies of articles to the hospitals, may be entrusted by the Board to a Committee, or Committees, consisting of their own members.
6. "The Board shall have power to purchase compulsorily such lands as they may require for the erection of hospitals for patients or convalescents; of disinfecting houses, and for all requisite purposes."
7. "Admission to the Board's hospitals shall not be considered as tending to pauperise the persons admitted." "No charge is to be made for treatment in the Board's hospitals; but the Board may make a reasonable charge for the use of separate rooms and special attendance for patients."
8. Patients may be admitted into the hospitals of the Board from outside their district, but in all such cases a reasonable charge for the accommodation shall be made.
9. "The Local Government Board shall have the power to constitute the Board a Sanitary Authority during the prevalence of an epidemic."
10. Various sanitary powers at present vested in sanitary authorities under the Public Health (Ireland) Act, 1878, shall be entrusted to the Board.
11. The Board may make by-laws, to be approved by the Local Government Board, breaches of such by-laws to be punished by a fine not exceeding £5.

12. The Board shall be authorised to pay clergymen for spiritual ministration to the patients.

The foregoing may, I think, be taken as a fair epitome of the twenty-eight clauses which make up the scheme. In criticising it, I propose to show that—

1. It is unnecessary.
2. It is inexpedient.
3. It would injure medical teaching in Dublin.
4. It would probably turn out a costly failure.

I. THE SCHEME IS UNNECESSARY.

Subject to correction by legal experts, I maintain that, in all essential particulars, Parliamentary powers already exist for carrying into effect the chief provisions of the scheme. The Public Health (Ireland) Act, 1878, Sections 137 to 148 inclusive, grapples effectually with the question of infectious diseases and provisions against infection. The prevention of the spread of infectious diseases is provided for in Sections 149 to 156 inclusive. It is true that the last-named sections require to be put in force by an order of the Local Government Board, but once in force they are comprehensive enough for all purposes.

Few will assert that the existing provisions in Dublin and its vicinity for coping with an outbreak and epidemic of small-pox like that of 1894–1895 are adequate or safe. For many years an urgent necessity has existed for the establishment of Convalescent Homes for patients recovering from infectious diseases, and of refuges for the temporary reception of the inmates of houses in which infection has appeared.

But all these defects can be remedied, all these wants can be supplied by enforcing the provisions of Sections 149 and 155 of the Public Health Act.

Listen to the wording of these sections:—

"149. Whenever any part of Ireland appears to be

threatened with or is affected by any formidable epidemic, endemic, or infectious disease, the Local Government Board may make, and from time to time alter or revoke, regulations for all or any of the following purposes; namely,

"(1) For the speedy interment of the dead; and

"(2) For house to house visitation;

"(3) For the provision of medical aid and *hospital accommodation*; and

"(4) For the promotion of cleansing, ventilation, and disinfection, and *for guarding against the spread of disease*; &c."

Surely in this section "Hospital Accommodation" includes "Convalescent Homes," as it does specifically in Section 155, to be presently quoted. And, again, the providing of refuges or shelters for persons exposed to infection is certainly a most effectual measure "for guarding against the spread of disease," and therefore the section may be construed as authorising the provision of such refuges or shelters.

"155. Any sanitary authority may, with the sanction of the Local Government Board, provide for the use of the inhabitants of its district hospitals or temporary places, for the reception of the *sick or convalescent*, and for that purpose may itself build such hospitals or *places of reception*," &c. This is tolerably explicit, anyone would think.

Then Section 153 provides that "The Local Government Board may, if they think fit, by order authorise or require *any two or more boards of guardians to act together* for the purposes of the provisions of this Act relating to prevention of epidemic disease, and may prescribe the mode of such joint action and *of defraying the costs thereof*."

It may be objected that the combination for the prevention of epidemic disease provided in Section 153 is limited to "boards of guardians." But Section 12 authorises the Local Government Board, by provisional order, to form any

sanitary districts, irrespective of kind or number, into an united district for procuring a common supply of water, or for making a system of sewerage, or *for any other purposes of this Act*, on the application of the sanitary authorities of such districts and after due inquiry.

And then as to "*the purchase and taking of lands otherwise than by agreement*" "*for the purposes of this Act*," Section 203 contains all requisite regulations, the "Land Clauses Acts" being incorporated with the "Public Health Act" for the purpose.

All this looks to me very like Sir Charles Cameron's scheme, ready, cut and dry, without the necessity of going to Parliament at all. But, apart from the legal question, is a gigantic scheme, such as that now proposed, really required in the interest of the Public Health? And here I have to complain of disingenuousness on the part of the promoters or advocates of the scheme. We are told that, in Dublin, almost alone of all the great cities of the United Kingdom, fever cases are treated *in* the general hospitals, that "in the metropolitan area of London, with a population of four millions, none of the hospitals treating infectious cases were in the city, but were erected under the most favourable topographical conditions that could be obtained." These are Sir Charles Cameron's words as reported in the *Daily Express* of October 11, 1895. In the same newspaper Mr. Antony Roche is reported to have said that "there was not a single case of small-pox treated in the city of London. Every case was sent 14 miles down the Thames. Sixteen thousand cases were so sent down, and the report as to these was that not a single one of these suffered from being sent that distance."

Now, surely Sir Charles Cameron is well aware that the London Fever Hospital stands in Liverpool-road, Islington. It is at least a mile and a-quarter from the open country,

whereas Cork-street Hospital and the Hardwicke Hospital are not one-third of that distance from the open country. I doubt not Sir Charles is perfectly familiar with the situation of the various Metropolitan Asylums Board's Hospitals, on which praise has been of late so freely lavished to the disadvantage of our Dublin Epidemic Hospitals. I take the list of these hospitals and their localities from *Burdett's Hospital and Charities Annual*, 1895, (pages 316, 317). They are:

Eastern Fever Hospital, The Grove, Homerton, E.
Fountain Fever Hospital, Lower Tooting.
Gore Farm Hospital, near Dartford, Kent. (For convalescent small-pox and fever patients.)
North-Eastern Hospital, St. Ann's Road, N.
Northern Fever Hospital, Winchmore Hill, N. (This is a Fever Convalescent Hospital.)
North-Western Hospital, Haverstock Hill, N.W.
South-Eastern Fever Hospital, New Cross, S.E.
South-Western Fever Hospital, Stockwell, S.W.
Western Fever Hospital, Fulham, S.W.
Small-pox Hospital Ships, Long Reach, near Dartford, Kent, containing 300 beds.

Now all these hospitals, except the Gore Farm Convalescent Home, and the Dartford Hospital Ships, are within the "inner London" district—that is, they are situated in populous localities. Mr. Roche's statement that every case of small-pox was sent 14 miles down the Thames will surely need some qualification when we read in the Registrar-General's Weekly Returns for August of the present year that, "according to returns from the Metropolitan Asylum and London Fever Hospitals and from the Highgate Small-pox Hospital," there were 336 cases of small-pox under treatment in these hospitals on Saturday, August 17, 357 cases on August 24, 341 on August 31, and 328 on Sept. 7.

Mr. Burdett tells us that the Small-pox Hospital Ships at Dartford contain 300 beds, so that, even supposing these beds were all filled on August 24, 57 small-pox patients must have been under treatment in either the Highgate Small-pox and Vaccination Hospital, or one of the Metropolitan Asylums Board's hospitals, which I have named above. I may add that about one-tenth of the beds at St. Thomas's Hospital are set apart for the treatment of "infectious cases" (52 out of 569 beds). (Burdett).

It is contended that Dublin is unique in having fever blocks attached to the general hospitals, and that these fever blocks are a menace to the public health. Let us consider these points for a moment. The case of Dublin is not comparable with that of any other large town in the United Kingdom. In Dublin there are as many as ten general hospitals, and only one of these—the Mater Misericordiæ—can be looked upon as a *large* hospital in the ordinary acceptation of the term. On the other hand, in Belfast and in the large towns of both England and Scotland, the general hospitals are few in number and contain many beds. The Royal Hospital, Belfast, contains 189 beds. The Royal Infirmary, Edinburgh, has 780 beds; the Royal Infirmary, Glasgow, 582 beds; the Western Infirmary, Glasgow, 400 beds; Manchester Royal Infirmary, 298 beds; Leeds General Infirmary, 394 beds; Liverpool Royal Infirmary, 295 beds; Liverpool Royal Southern Hospital, 200 beds, and so on. Now, I am not defending the Dublin system—rather do I condemn it as wasteful and tending to retard medical progress. But the hard, the stubborn fact remains—this system, however objectionable it may be, exists; and to abolish it would be infinitely costly, while the substitution of two great infirmaries would be most difficult to effect, because of the multifarious vested interests involved. Theoretically, I am in favour of a great reform which would give Dublin two large,

fully-equipped, and richly-endowed Royal Infirmaries in lieu of the many existing hospitals, and two Epidemic Hospitals on the same scale instead of the present fever hospitals and fever wings of the general hospitals. But to carry out such a scheme would entail an expenditure of probably a million of money sterling by the time vested interests were provided for, sites procured, buildings erected, and the management in working order.

Setting aside so great a scheme then, I ask—Are we prepared to cripple the teaching power of the existing clinical hospitals in Dublin by excluding fever cases from their precincts on the ground that infection spreads from the epidemic blocks to the main hospital buildings? Is this allegation proved?

My long experience convinces me that such an occurrence is altogether exceptional—so exceptional as to warrant us in altogether discounting it in practice. I believe it is true that scarlet fever or measles has now and again broken out in the wards of our general hospitals, but such an accident has happened twice within the past few weeks in two general hospitals in Dublin, which have no fever wings; so, at all events, I am informed. How this is brought about is easily understood when it is remembered that every second day or so crowds of visitors throng the wards. Then, if an epidemic is rife in the population at large, it is apt to be introduced into a hospital on visiting days.

II. THE SCHEME IS INEXPEDIENT.

Dublin is unique not only in possessing many general hospitals, but also in receiving an annual grant from the Imperial Parliament, in aid of the funds of those hospitals. This grant amounts to £15,850 per annum. It is distributed as follows:—

House of Industry Hospitals - -	£7,600
Steevens' Hospital - - -	1,300
The Meath - - - -	600
Rotunda Lying-in Hospital - -	700
The Coombe Lying-in Hospital - -	200
The Westmoreland Lock Hospital -	2,600
Cork-street Fever Hospital - -	2,500
St. Mark's Ophthalmic Hospital -	100
The Royal Hospital for Incurables -	250
	£15,850

In a letter to the *Lancet*, dated November 2, 1895, Sir Charles Cameron somewhat apologetically states that in the draft scheme no reference is made to the existing Dublin Hospitals, and that a motion made at the Conference to consider the question of the public money grants made to them, was spoken against by him and was rejected. But Sir Charles will scarcely deny that this question was *directly* raised at that Conference by more than one speaker, and that it was bound to be raised *indirectly* sooner or later in connection with his scheme.

For example, I ask is it at all likely that this large Parliamentary grant of some £16,000 a year will be continued if a Hospital Bill should be laid before Parliament? There is no precedent for such a grant, in the first place; and again, if fever is no longer treated in the Hardwicke or Steevens' or the Meath—not to mention Cork-street Hospital—one chief claim of these institutions upon the liberality of Parliament will be entirely forfeited.

But the scheme is inexpedient from another point of view—that of the Public Health. The Council of the Dublin Sanitary Association have very pertinently shown that the dislike of patients to go, and of parents to send their children, to hospitals situated at a distance and specifically called

"isolation" or "fever hospitals," would lead to concealment of infectious disease and its consequent spread in the homes of the poorer classes. The editors of the *Lancet*, in commenting upon the views of the Council of the Dublin Sanitary Association, think that this objection is "one which carries but little weight." In a letter addressed to the editors and which was published in the *Lancet* of November 2, I say: "I cannot doubt that this is really your deliberate opinion, but possibly your experience lies among the Anglo-Saxon rather than among the Irish poor. For thirteen years I acted as physician to Cork-street Fever Hospital; for more than twenty years I have been physician to the Meath Hospital, and from this experience I emphatically dissent from your conclusion. The lower orders among an Irish population dread the very name of a Fever Hospital, and the difficulty of inducing parents to send their sick children to hospital is enormously increased when the order is for a fever hospital, and not for the fever wing of a general hospital. I gladly accord my tribute of praise to the present resident medical officers of the Cork-street Fever Hospital for the manner in which, by tact and kindness, they have within recent years succeeded in making that institution comparatively popular; but Dr. J. Marshall Day and his colleagues will, if I mistake not, bear me out in what I have stated.

III. THE SCHEME WOULD INJURE MEDICAL TEACHING IN DUBLIN.

The editors of the *Lancet*, in the article to which I have already alluded, observe:—"The third objection to the scheme which has been formulated is to the effect that the proposed scheme would very injuriously affect the interests of the Medical School of Dublin by interfering with the teaching of the infectious fevers and of their treatment by the

clinical physicians of the city, and that in this manner obstacles would be thrown in the way of students. This, we frankly admit, is a real objection. The removal of the infectious fevers from the London general hospitals has tended in the same direction; and in order to meet it as far as possible it has become necessary to require, as a condition to admission for examination for a diploma to practise, that every student should have had special tuition in this respect in a hospital especially set apart for the reception of fever cases. But the difficulty is not one which applies to Dublin alone, and we may be certain that the physicians and teachers in the London hospitals would never have consented to *the removal from their hospitals of the majority of the fevers, so necessary for teaching purposes*, had there not been some altogether overwhelming reason for submitting to it. That such reason existed is perfectly well known, and it is highly probable that its application is by no means limited to the English metropolis. . . . The only question is whether, in Dublin as elsewhere, the evil of reducing facilities for clinical teaching is not more than compensated for by advantages to the public and to other hospital patients, and whether it cannot be made good by other teaching arrangements."

Now, in reply to all this, I venture to point out that hardly one of the London hospitals has any considerable air-space, or "lung," around it, within the limits of which a detached fever wing could be built. and, as a matter of fact, no such fever wings exist, except, I believe, at St. Thomas's Hospital. Therefore, it was in London really a question of treating fevers actually in the general hospitals or excluding them altogether. In Dublin, on the other hand, most of the hospitals stand on their own grounds, which are more or less extensive, and so permit of the erection of detached fever-blocks at a fair distance from the main buildings.

But, further, surely it was a smaller sacrifice for a London

clinical hospital with an average of 404 beds to give up its fever-beds, than it would be for a Dublin clinical hospital with its average of 168 beds, *including* those intended for fever cases, to close the latter?

London.		Dublin.	
Charing Cross	175	Adelaide	135
Guy's	600	City of Dublin	120
King's College	217	Steevens'	230
London	776	House of Industry	322
Middlesex	321	Jervis Street	100
Univ. College	208	Mater	323
St. Bartholomews'	745	Meath	116
St. George's	351	Mercer's	92
St. Mary's	281	St. Vincent's	165
St. Thomas's	569	Sir P. Dun's	80
Westminster	205		
	4,448		1,683

Both the Lord Mayor of Dublin and Mr. Beardwood, the Chairman of the Public Health Committee of the Corporation of this city, have, during this controversy, ventured to assert that the opposition to the Infectious Hospitals Scheme has originated with the medical profession, that it is an interested opposition, and shall not therefore be taken into account. I tell these gentlemen that even if the opposition to the scheme had come from the medical profession alone it should command the respectful attention of the most enthusiastic sanitary reformers. There is a great medical school in Dublin. It has been estimated that it is worth £100,000 a year to the citizens. Many causes have combined within recent years to lessen the number of the students of that school. Let us not imperil its very existence. It would be bad enough to substitute salaried medical officers for the independent clinical physicians who at present attend fever cases in the epidemic wings of the general hospitals. It

would be nothing short of a disaster to the public as well as to the profession if medical students were discouraged from studying fevers by such a change of teachers and by the removal to a distance of their "fever cases." The proposed epidemic hospital, or hospitals, will be in the "County Dublin." How are students to attend at a distance from their medical school and general hospital? Both time and money will fail them, and so they will not learn fever and its treatment.

At another time and in another place I wrote:—"It has always seemed to me that each individual case of fever presented to the attending physician or to the clinical student an epitome of the Principles and Practice of Medicine. 'Fever,' said Fordyce, nearly a century ago, 'is a disease which affects the whole system; it affects the head, trunk and extremities; it affects the circulation, absorption, and the nervous system; it affects the body, and it affects the mind; it is, therefore, a disease of the whole system, in the fullest sense of the term.' This being so, we surely are in a position to study diseases of the three great cavities of the body, diseases of the circulation, of the lymphatic, digestive, and nervous systems in an individual case of fever. If our object is—as it should be—to accurately gauge the state of our fever patient, we cannot attain this end by a mere superficial examination. On the contrary, every known method of physical examination must be applied, and this, too, as regards every system of the body in rotation. In a word, there is no other disease which demands on the part of the physician a closer and more intelligent observation, a more minute and searching physical examination, a more subtle and refined train of reasoning, a more careful weighing of evidence for or against, and a more conscientious and painstaking management from start to finish. It is therefore plain that in future a personal study of fever will be an indispensable part of medical education.'

IV. THE SCHEME WOULD PROBABLY TURN OUT A COSTLY FAILURE.

It is really almost superfluous to discuss this point with the history of this ill-fated scheme before us. *It has already proved a failure, and to the reputation of its promoters a costly failure.* At a meeting held on October 10, and presided over by the Lord Mayor of Dublin, its provisions were explained to representatives of the public bodies concerned, all of whom were very careful to make it perfectly clear that they were present to hear, but not to vote for or against, the scheme. They went to the meeting invested with neither power nor authority by the bodies of which they were delegates. Several of these bodies have since that meeting considered the scheme and repudiated it. The Board of Guardians of the Rathdown Union *have refused to co-operate in any way in the scheme.* The South Dublin Union Board of Guardians have thrown cold water upon it. The Kingstown Township Commissioners have decided to take no further action with reference to it. The Commissioners of the important and wealthy Pembroke Township do not approve the scheme, and have refused to send representatives to serve on a sub-committee appointed to carry out details. Thus one-half of the "County Dublin" has already declared itself dead against the scheme, which is a "City of Dublin" scheme from beginning to end.

But the *coup de grace* has been given by the infanticidal hand of Sir Charles Cameron himself. At the weekly meeting of the Board of Guardians of the South Dublin Union, on Thursday, November 7th, the Clerk read the following remarkable letter from Sir Charles Cameron:—

"I would like to point out that the promoters of the scheme for establishing an isolated hospital had no intention whatever to interfere with existing hospitals or the grants made to them. Their principal objects—were (1) The establishment of a convalescent

home for patients recovering, or recently recovered, from infectious diseases; (2) to provide proper accommodation for smallpox patients. The recent epidemic shows that there is not at present proper accommodation for such patients, and there are few who will be disposed to dissent when I say that the precincts of a workhouse are not the proper place in which convalescents from smallpox should be located. Many persons of most respectable positions in society were reluctantly obliged to go into the workhouse sheds set apart for smallpox cases last year. I trust that in the interests of the public your Board will appoint two delegates to consider this important subject at the next conference in relation to it which may be held. It is, perhaps, right to point out that the Corporation have the power to establish themselves smallpox hospitals and convalescent homes for infectious cases."

Furthermore, in his letter to the *Lancet*, which I have already quoted, Sir Charles says that, at the conference on October 10, "it was distinctly stated that the scheme could be modified in any way provided that provisions were made in it for establishing one or more hospitals for small-pox and a home for convalescents from infectious diseases outside the city."

If this is Sir Charles's present attitude in regard to his scheme, we may well say:—"Much cry and little wool."—

"Parturiunt montes, nascetur ridiculus mus."

According to the views expressed in these letters the proposed scheme "would, in effect"—to adopt the words of the Council of the Dublin Sanitary Association—"have reference to one disease only, and that a disease absolutely controllable by periodical vaccination." At the meeting on October 10, the Lord Mayor expressed his opinion that the up-keep of the proposed infectious (smallpox) hospital would cost £10,000 a year, and that to it the Government grant "might be transferred." At the same meeting Mr. Kelly said he thought that "one large hospital would be unsuitable, as great difficulty and danger would be experienced in removing

patients from outlying districts north and south of Dublin. *The whole scheme was too ambitious, and must fall necessarily by its own weight.* He believed he was speaking the opinion of the small community he represented (Blackrock) when he said that they to a man would oppose the scheme, even irrespective of the further taxation which would be its inevitable result."

Such was the temper in which the scheme was received on its *début.* Is it any wonder that its promoters are already "drawing in their horns," and that the scheme itself may be registered as "still-born ?"

To sum up:—While I entirely disapprove the draft scheme prepared by the Superintendent Medical Officer of Health on the grounds that it is unnecessary, inexpedient, calculated to damage the Medical School of Dublin, and likely to prove a costly failure, I freely admit that the accommodation for small-pox patients in the late epidemic was neither adequate nor safe. Again, for twenty years I have been advocating the provision of refuges, or temporary habitations, for the reception of the inmates of houses in which infectious disease has broken out, and of convalescent homes for those recovering from infectious diseases. So far I am heartily in accord with Sir Charles Cameron.

Let then the Urban and Rural Sanitary Authorities of the Dublin Metropolitan District combine under Sections 12, 149, and 155 of the Public Health (Ireland) Act, 1878 (41 and 42 Vic., Cap. 52), to carry out these needed sanitary reforms. But let us have no attempt at Parliamentary legislation, which is unnecessary, and no scheme like that proposed —a scheme revolutionary, cumbrous, unwieldy, unpractical, and unworkable, which in one short fortnight was spurned by half the people who were to work it.

ON THE ISOLATION OF FEVER PATIENTS.

By SIR CHARLES A. CAMERON,
Professor of Hygiene, Royal College of Surgeons;
Chief Medical Officer of Health for Dublin.

[Read at a meeting of the combined Sections of Medicine and State Medicine, November 15, 1895.]

It is admitted that some infective diseases, such as small-pox, scarlet fever, and typhus fever, are propagated through the medium of infected air. It is evident, therefore, that persons suffering from such diseases should be kept as far apart from healthy persons as circumstances admit of. The *materies morbi* of these maladies consist of solid bodies, which, though capable of being blown about in the atmosphere, ultimately settle upon the ground, floors, shelves, walls, animals, and, in a word, on the surface of things generally. If they settle upon our clothes, the latter may convey them to great distances; and if persons reside close to the place where a small-pox or typhus fever patient is located, the air itself may waft the poison of the disease directly from the sick to the sound.

The precautions taken in modern times to reduce the points of contact between fever patients and healthy persons have been a potent factor in reducing the zymotic death-rate, and have done almost as much in that direction as have these general hygienic measures for the prevention of disease so largely adopted within the last thirty years.

The combined efforts of medical and non-medical men in the departments of Preventive Medicine and Public Hygiene have largely reduced the death-rate in these countries. As might be expected, these efforts have produced a greater

effect as regards zymotic diseases than in relation to other maladies. In the period 1876–1880 the zymotic death-rate in the large English and Welsh towns was 3·8 per 1,000 persons living, whilst the death-rate from all other causes was 21 per 1,000. In the following decade the zymotic death-rate declined to 3·1 per 1,000, being 18·4 per cent. less than in the previous five years; whilst the death-rate from all other causes fell to 19·85, or 5·5 per cent. less than in the previous five years. In the four years ended in 1894 the zymotic death-rate was 2·66, and the death-rate from all other causes 18·31, showing a decline of 14·2 per cent. of the former and 7·7 per cent. of the latter, as compared with the previous ten years.

In Dublin the greater decrease in the zymotic death-rate, as compared with the decrease in the general mortality, is still more striking. In the ten years ended in 1890 the zymotic death-rate was 44·05 per cent. less than in the previous ten years, whilst the decrease in the number of deaths from all causes was only 15·59 per cent. In the five years ended in 1880 the zymotic death-rate was 5·1 per 1,000 persons living, whilst the death-rate from all other causes was 27·5. In the four years ended in 1894 the zymotic death-rate was 2·55, or 50 per cent. less than in the five years 1876–80, whilst the death-rate* from all other causes was 23·37, or 15 per cent. less.

These statistics seem to prove the possibility of the extinction of the principal zymotic diseases. If the term "preventable," which is sometimes applied to them, be a true one, then we are not irrational in our attempts to prevent their occurrence.

It must be admitted that the great decrease in the mortality caused by fevers is largely due to the enlightened

* The total death-rate for English towns is corrected for age and sex distribution; but these corrections have not been applied to the Dublin deaths.

action of bodies composed almost wholly of non-medical men. Our sanitary laws are the work of the Legislature, and the enormous sums which have been expended on the improved sanitation of towns have been voted by Municipal Authorities. The medical profession have played a useful and noble part in suggesting means for the prevention of disease; and to the action of the Medical Officers of Health of the eastern English ports we are, almost to a certainty, indebted for our immunity from Asiatic cholera during its two latest invasions of Europe. The English Medical Officers of Health have also rendered great service to the public by their persistent efforts to secure properly isolated hospitals exclusively for infectious cases. In 1891 Dr. Thorne Thorne, now Medical Officer of the English Local Government Board, called attention to the remarkable increase in the provision by Sanitary Authorities of hospitals for fever patients. From 1879 to 1891 £450,000 had been expended in the erection of these hospitals, and it was estimated that there were at least 400 of them in existence. The Corporation of Belfast have just resolved to provide a hospital of the kind outside their city, and at an expense of £70,000.

Dublin has a reputation as a great seat of medical education, and its physicians have done much to enlarge the boundaries of the domain of medicine; but I do not think that we can boast of any superiority in our hospital system. It must be admitted that the number of hospitals in relation to population is far greater than in probably any city in the world. Still I do not consider this is a fact much to be deplored. Every hospital has a large committee, whose efforts to maintain its efficiency and resources are incessant, and generally successful. If, instead of a dozen hospitals, we had only two, it is doubtful whether or not their income would be as great as the total incomes of the dozen hospitals. The great defect in the system is the treatment of the

highly-contagious fevers in the general hospitals. In addition to two special fever hospitals, there are fever departments in six general hospitals and in one private hospital. I cannot understand how anyone claiming to be a sanitarian can approve of the treatment of the highly-contagious fevers at nine different points in a comparatively small city such as Dublin is.

We have a great maternity—the Rotunda Hospital—which for more than a century has been a model for the world to imitate. I fear it must be admitted that this is the only one of our hospitals which can justly claim a superiority over others. I trust that the day will come when our general hospitals will, like those on the other side of the Channel, refuse to admit small-pox, scarlet fever, typhus fever, and perhaps measles. When they have come to this conclusion, they will have removed what Mr. Burdett terms the great blot on the Dublin hospital system.

The only excuse that can be properly made for the reception of fever cases in general hospitals is the want of a special fever hospital; but no such excuse can be urged in Dublin, for there have long been two fever hospitals in this city. Such being the case, there was no necessity for treating fever in other hospitals. It seems to me that Cork-street Hospital should have been more liberally supported by the people of Dublin. Originally in a thinly-populated district, it was placed under conditions something like those now insisted upon in the case of isolation hospitals; now it is surrounded by dwelling-houses. It is to be regretted that the authorities of this hospital—which has done right good work—were not enabled long ago to acquire a considerable area of the ground surrounding it, and upon part of which dwelling-houses have been recently erected. Hemmed in by houses, it can no longer be regarded as a thoroughly isolated hospital—certainly not so far as small-pox is con-

cerned. As it is, Cork-street Hospital is the best place we have in Dublin for fever cases, as it receives no other kind, and has still some open space surrounding it.

We have in the Hardwicke our second fever hospital. It is quite unsuited for the reception of small-pox cases; and even as a hospital for scarlet fever and typhus fever, it cannot for a moment be compared with the vast majority of similar hospitals in England and Scotland.

I am aware that many physicians believe that there is sufficient reason for having fever wards in general hospitals; but I challenge them to cite a single author on hospital management, or on public hygiene, in support of their belief. To a certain extent the authorities of the Dublin general hospitals acknowledge the desirability of isolating fever patients, as some of them have removed their fever wards to detached buildings, which are, however, too close to the main buildings. They are under the control of the general hospital administration; the same physicians and students who attend the wards in which non-infectious cases are treated visit the fever wards. It is not to be expected that the nurses in the detached building keep apart from those who attend in the main building. The pupil who at 10 o'clock a.m. may be standing over a typhus fever patient, may, an hour later, be witnessing an operation in the surgical theatre.

In the ideal hospital for infectious maladies, the physician's clothes are changed before he leaves it; so, also, are those of the pupils, officers, nurses and attendants. They wash the portions of their body exposed to the infected air. The visitor is covered with an ample cloak, and is told not to enter a public conveyance immediately after leaving the hospital. The goods for the hospital are received at the porter's lodge. The hospital is surrounded by spacious grounds, and is not nearer than 150 feet from the public

road. The *dejecta* of the patients are destroyed by fire, or sterilised by high-pressure steam. Is the fever department of a Dublin hospital placed under these conditions?

The recent epidemic of small-pox—of which we may expect a recrudescence very soon—showed up our weakness as regards isolation hospitals. Cork-street and the Hardwicke proved insufficient to accommodate the patients, and it was found necessary to remove the less acute cases to huts situated within the precincts of the Workhouse. There was no proper convalescent home in which those recovered from the disease, but still possibly not free from infection, could be accommodated. Many of the patients went into the Workhouse under protest. We cannot wonder that they should object to be treated as if they were paupers.

The recent epidemic of small-pox found us very badly equipped to meet it clinically. The Public Health Committee, early in the epidemic, pressed the Boards of Guardians to erect temporary hospitals in isolated positions; but nothing was done in that direction until the disease had firmly established itself. Cork-street and Hardwicke Hospitals had to be used. Both were completely filled with patients; and then, tardily, the South Dublin Union provided huts close to the Workhouse, into which at once a large number of the less acute cases and some convalescents were drafted from the hospitals. On the 7th of January, 1895, there were 122 small-pox cases in the Hardwicke, 153 in Cork-street, and 61 in the Workhouse Sheds—total, 324. On the 19th of the same month there were 51 cases in the Hardwicke, 154 in Cork-street, and 154 in the Workhouse. The sheds at the latter place were very defective; but those erected at a later period were much better.

As regards situation, the sheds were in a somewhat better position than Cork-street Hospital, and the latter than the Hardwicke. The two last named hospitals are quite close to

public thoroughfares. Let us see what is the opinion of eminent authorities as regards the situation of a small-pox hospital. Dr. Wynter Blyth, in his work on Public Health, says that they should be a mile distant from dwellings. Dr. Priestley, Medical Officer of Health for Leicester, demonstrated, in 1893, that the Leicester small-pox hospital, though 620 feet distant from the nearest house, spread the disease, and he recommended that the hospital should be removed "as far away as possible from the town and its inhabitants." Dr. C. E. Matthews, Superintendent of the small-pox hospital at Gore Farm, considers that 1,000 feet are the limits of the aërial infection of small-pox; but this estimate has been questioned. Dr. Stafford, Medical Inspector of the Irish Local Government, has published in the last Annual Report of the Board very striking English statistics, showing that small-pox hospitals spread the disease unless they are thoroughly isolated and well managed.

If hospitals are likely to spread small-pox unless they are placed under proper conditions, I cannot see why they should not spread typhus fever, scarlet fever and measles, which are acknowledged to be propagated through the air, and by the medium of clothes, &c. I admit that small-pox virus travels through greater distances, and that the influence of hospitals in spreading this disease has been far more clearly proved than is the case with respect to other zymotic diseases. It is, therefore, not so necessary to place ordinary fever hospitals very remote from towns; but still they should be separate hospitals, and have ample space around them. Dr. Wynter Blyth, in his work on Public Health, quotes with approval the summary made by Dr. Thorne Thorne, the present Medical Officer of the English Local Government Board, of the opinions of eminent physicians and epidemiologists, such as Dr. Bristowe, as to the means of preventing contagion. He says:—

"The first principle to be laid down is the absolute necessity of buildings set apart for small-pox, scarlet and other infectious fevers. A Sanitary Authority not providing facilities for isolation, has failed to fulfil one of the great purposes for which it was instituted. The second principle is, that the construction of such hospitals is best carried out with deliberation in non-epidemic times, experience having demonstrated that those which have been hastily run up, to meet a sudden emergency, have provided accommodation of a most indifferent kind, have failed to meet the permanent want of a district, and the cost has been, in relation to usefulness, large. The third principle is, that small-pox hospitals should be at least a mile from inhabited dwellings."

Dr. John W. Mason, Medical Officer of Health for Hull, and physician to the fever hospital—one of the ablest sanitarians in England—says that the number of patients in a fever hospital should not exceed 20 per acre. What general hospital in Dublin gives an acre to 20 of its fever patients? He states that each zymotic disease should be treated in a building aërially distinct from every other.

No stronger condemnation of the system of having fever departments in general hospitals could be made than that passed on it by one of the most eminent physicians and sanitarians of these countries—Dr. Grimshaw, Registrar-General, and the present President of the Royal College of Physicians of Ireland. Dr. Grimshaw writes:—

"I have already shown that we have abundance of general hospital accommodation for both our pauper and our artisan classes. I think we may say we have ample accommodation for lying-in women. We have, I believe, an insufficient amount of accommodation for infective fevers, and not only is what we have insufficient, but some of it is, I believe, of a character unsuitable for the patients, and dangerous to the community at large. Fever hospital accommodation, as it is popularly called, requires to be provided in great institutions: each form of fever having its specially isolated wards. A good many of these conditions are fulfilled at Cork-street and the Hardwicke Fever Hospitals; but, I am sorry to say, a system is still in existence of providing fever wards in connection with our general hospitals—

a system which, I think, cannot be too strongly condemned. Every fever hospital is a danger to its neighbours, and no matter what precautions may be taken, it cannot be denied that each fever hospital, or fever ward of a general hospital, is a focus from which fevers may spread to the neighbourhood. There is no doubt that fever has spread from fever wards in general hospitals to the general medical and surgical wards. No matter how perfect the rules, or how well organised the supervision, or how efficient the management, we may at any time be at the mercy of a careless or ignorant person, who may be but temporarily in the employment of the best managed institution.

"It is essential to safety that well regulated and well isolated hospitals for the treatment of infective diseases should exist in every district, and that all attempts to deal with these forms of disease in general hospitals should be abandoned. A recent investigation by the Medical Department of the Local Government Board of England proves to demonstration the risks attendant on the proximity to a small-pox hospital. The alarm raised by the establishment of small-pox hospitals in populous neighbourhoods led to an inquiry, which has been carried out in the London District by Mr. Power, one of the Inspectors of the Medical Department of the Local Government Board of England, the general result of which is stated as follows by Dr. Buchanan, the Medical Officer of the Local Government Board of England:—

"'But they need only be studied by the light of the Fulham experiences to give a presumption (apart from any afforded by any local health officers' reports) that those experiences of Fulham have been repeated at Hampstead, at Homerton, at Stockwell, and at Deptford, at each epidemic period—a presumption, namely, that small-pox infection has habitually been distributed from each of those hospitals over considerable areas, most conspicuously at the commencement of epidemic periods; with a further presumption that this distribution has greatly influenced the small-pox mortality of whole registration districts; while incidental illustrations are not wanting of peculiar excess in the incidence of small-pox upon portions of districts nearest to the several hospitals. As regards the method of dispersion, Mr. Power (without considering it in terms) supplies me with reasons for believing that to whatever degree limitations in number of patients, and improvements in administration and in ambulance service, may have operated to reduce the danger of small-pox hospitals to their

neighbours, these changes have had but a partial result; and a hypothesis of atmospheric convection over considerable distances is wanted, not for Fulham alone, but for London generally, in order to explain the observed behaviour of small-pox in the several districts of the metropolis.'

"This then points to risks which I firmly believe are not confined to small-pox, but are in a more or less degree attendant on all forms of infective disease, and which will become increased in proportion to the number of centres of contagion established. I believe it will be found necessary to be much more careful than we have hitherto been in selecting sites for our fever hospitals, and in effectually providing against their becoming centres of infection and promoting disease in one place, while preventing it in another. We must have ample space around our epidemic hospitals, plenty of accommodation for our convalescents; we must intercept and disinfect our sewage; and from the evidence recently obtained in London, it seems necessary to disinfect the air which escapes through the hospital ventilators, before we again allow it to be inhaled by healthy people. The latter may seem an extreme hygiene expedient; but I believe we have evidence now before us which shows that such precautions cannot be disregarded."

It has been said that the establishment of special hospitals for fever would greatly interfere with medical education in Dublin. This is a new idea. Has Cork-street Fever Hospital retarded medical education? No one ever said that it did. It seems to be taken for granted that the scheme for establishing isolation hospitals contemplates putting them many miles outside the city. No such intention was in the minds of the promoters of the scheme. The hospitals were to be outside the city, but close to it, and with ample space surrounding them. The small-pox hospital need not be more than one mile distant from the Circular-road.

To my mind the present hospital system in Dublin is unfavourable to the study of fever. The cases are treated in nine hospitals, and, with the exception of the special hospitals, there cannot be very many cases in any one of the hospitals. The proper thing would be for the pupil, in his fifth year, to

devote himself to the study of fever in a large special hospital such as Cork-street, containing, perhaps, ten times the number of patients treated in his general hospital. In the latter he would have in his earlier years the opportunity of the clinical study of enteric fever and pneumonia. It seems to me highly desirable that, whilst occupied in the study of the highly contagious fevers, the pupil should keep apart from the general hospital.

An exaggerated notion of the expense of establishing isolation hospitals prevails. The fever hospital, a mile outside Newcastle, cost, including everything, £26,662; it stands on 11 acres, and contains 105 beds. Nottingham fever hospital cost £25,000; its grounds comprise 12½ acres, and it has 140 beds. The fever hospital, situated more than a mile outside Hull, cost only £15,000; it stands on 7½ acres, and contains 150 beds. The usual estimate as to the accommodation which should be afforded in fever hospitals is one bed per 1,000 of the inhabitants of the district using the hospital. As Dublin City has a population of 245,000, 245 beds are required, according to the above estimate; but as there are no fewer than eleven hospitals in which enteric fever and pneumonia are, and might continue to be, treated, one or two isolation hospitals, having 140 beds, would suffice for this city.

During the ten years ended in 1894, there were on the average, in the last week in June, the following numbers of patients suffering from fevers in the principal Dublin hospitals:—

Measles	31
Scarlet Fever	31
Typhus Fever	6
Enteric Fever	28
Pneumonia	13
Total ...	109

In the same period, but in the last week of the year, there were:—

Measles ...	...	...	11
Scarlet Fever	...	...	47
Typhus Fever	...	...	10
Enteric Fever	...	...	75
Pneumonia ...	...	...	13
	Total,	...	156

If we omit enteric fever and pneumonia, the principal Dublin hospitals contained, on the average, 68 fever patients in the last week in June, and the same number in the last week in December. The maximal number of patients in the last week of June or December, in the above-mentioned ten years, was, excluding enteric and pneumonia cases, 169, of which 130 were measles cases.

In these statistics no reference is made to small-pox, which disease should be treated in a special hospital.

When Dublin is supplied with proper isolation hospitals for the treatment of the more contagious fevers, with a special hospital for small-pox, and with a home for convalescents recovering from fever, than we may feel that Mr. Burdett can no longer point to a great blot on our system of tending the sick.

TABLE.—*Number of Cases of Infectious Diseases in the Principal Dublin Hospitals in the Last Weeks in June and December in each year*—1885 *to* 1894.

LAST WEEK IN JUNE.

Diseases	1894	1893	1892	1891	1890	1889	1888	1887	1886	1885	Mean 1885–94
Measles - -	6	16	56	13	6	56	7	130	0	21	31·1
Scarlet fever - -	47	64	14	9	1	20	38	34	49	35	31·1
Typhus - -	0	5	4	5	3	7	10	5	6	16	6·1
Enteric - -	57	30	39	34	39	30	17	11	15	8	28·0
Pneumonia - -	21	20	23	11	13	5	8	9	8	10	12·8
Total, - -	131	135	136	72	62	118	80	189	78	90	109·1
Enteric fever and pneumonia - -	78	50	62	45	52	35	25	20	23	18	40·8
Measles, scarlet fever, typhus fever -	53	85	74	27	10	83	55	169	55	72	68·3

LAST WEEK IN DECEMBER.

Diseases	1894	1893	1892	1891	1890	1889	1888	1887	1886	1885	Mean 1885–94
Measles - -	1	57	9	3	9	9	5	18	0	0	11·1
Scarlet Fever - -	79	93	15	9	2	8	29	77	136	22	47·0
Typhus - -	1	7	2	5	16	15	4	20	10	16	9·6
Enteric - -	67	76	161	115	70	108	64	17	38	29	74·5
Pneumonia - -	9	24	25	28	13	8	5	6	5	10	13·3
Total - -	157	257	212	160	110	148	107	138	189	77	155·5
Enteric fever and pneumonia - -	76	100	186	143	83	116	69	23	43	39	87·8
Measles, scarlet fever, typhus fever -	81	157	26	17	27	32	38	115	146	38	67·7

Dr. James Little said that Sir Charles Cameron had put the theory of infectious disease very strongly and plausibly. The force of those arguments was, however, greatly lessened to himself by his own experience. For the last thirty years he had been connected with the Adelaide Hospital, where they have always taken infectious diseases of all kinds except small-pox. During that time he had known only two patients who contracted an infectious disease in the hospital. One was a patient with enteric fever, who caught typhus. The other was also suffering from enteric and he caught scarlatina. In each case the patient that gave the disease was lying in the next bed. There had never been a spreading of scarlatina from the fever hospital to the main hospital, which is a few yards from it. For many years it had been his practice when beginning his clinical teaching, to go with all the class to the scarlatina ward, and then next to the children's ward, and no child got scarlatina in that ward. He did not do so now. In the fever hospital the upper floor is for scarlatina, the middle for enteric, and the lower, the observation wards. The nurses and students go up from one floor to the other and the patients never contract scarlatina. It was his opinion that patients ill of infectious diseases should not be put lying next patients ill with other diseases. He would not mix up scarlatina with enteric or pneumonia. In Dublin many parents would willingly send their children to a certain hospital because they know some of the staff, but would conceal the fact of the child's illness in order to prevent his removal to a fever hospital outside the town. He thought that students would learn a great deal more about fever by carefully watching one or two cases than by going into a large fever hospital. According to Sir Charles Cameron, there is a great decrease of fever cases under the existing circumstances, and so he did not know why they should be changed.

Mr. Tobin said there was a great necessity that students should be thoroughly instructed in each variety of fever.

Dr. Walter Smith signified his unqualified approbation of what Dr. Little had said. It was perfectly absurd to talk of theory as against the enormous mass of practical facts. Besides, the finances of Dublin must be considered. It would inflict an irreparable loss on the education of students. How was the proposed new hospital to be always kept a mile outside the town? How were people to be prevented from building around and up to it? A very constant use of the present fever wing was made by the surgeons

sending their cases of erysipelas there. If there were no fever wing, how would the erysipelas cases be isolated? Small-pox was much more contagious than the other fevers. Typhus, which came next, occurred here only epidemically.

Dr. C. F. Moore said that as a student he had seen only one case of small-pox. Shortly afterwards he had to make a diagnosis, on board a large steamer, whether a certain case was one of small-pox or not. If he had not seen the one case, he would not have been so conversant with the disease. He thought small-pox was spread, not from Cork-street Hospital, but from the public-houses in the neighbourhood. Another agent to produce small-pox in the locality was the mountain of filth, forty or fifty feet high, in Marrowbone-lane, where the Corporation carts emptied all the rubbish. So he did not think the hospital should be blamed for the origin of small-pox in that neighbourhood.

Dr. S. M. Thompson said that during the late epidemic of small-pox, 300 beds were taken from the South Dublin Union, greatly to the detriment of patients who were very ill. Between four and five thousand pounds were spent in erecting sheds of wood, which should have been of concrete, and which were quite inadequate. It was absolutely essential when other hospitals would not take in small-pox that a small hospital should be made for their reception, and that this should be done now and not during an epidemic. Asiatic cholera was also very fatal and contagious.

Surgeon-Major Daly proposed that the Pigeon House Fort should be utilised for the purpose. It was a mile distant, and was by the water-side. There would be no building expenses.

Dr. C. Joynt, with 30 years' experience in India, never knew a single case of infectious disease to be conveyed in a hospital.

Dr. Craig said that Dr. J. W. Moore had very clearly laid before them the objections raised. He did not think they should consider the cost of the hospital if it was good for the public health. With regard to the interference with medical teaching, he would not discuss that, as it would not carry much weight with either the public or the Corporation. All who had any experience knew that the friends of those suffering from infectious diseases had a great objection to sending them into a fever hospital. If not able to send them into the fever wing of a general hospital they would keep them at home, and a great deal of concealment would go on. Dr. Moore had shown them that the Public Health Act contained enough to enable the Local Government Board to provide a small-

pox hospital and a convalescent home, and all were agreed that these were necessary. The people must be taught that small-pox was preventable by proper vaccination, and re-vaccination, and sufficient force was not laid on this point. The spread of typhus could also be prevented by better sanitation. Refuges for the temporary accommodation of the inmates of houses in which small-pox had broken out were also needed.

Sir Thornley Stoker wished to say a few words from a surgeon's point of view. It appeared to him that to deal with the question of the proposed hospital there should be a hospital for cholera, for small-pox, and for patients convalescent from infectious diseases. He believed that it was practically an impossibility for an outbreak of cholera to occur again. It would be waste of money to meet an epidemic which it is extremely improbable will ever enter this country. He was entirely in accord with the previous speakers, that the proper course with regard to small-pox was its prevention, and not its cure. He agreed with Sir Charles Cameron on the necessity for a convalescent home for patients getting well from infectious diseases. He was not aware that fever found its way into the Richmond Hospital, nor did they ever find any inconvenience from the small-pox when it was treated in the Hardwicke Hospital. He thought that one fever, to a certain extent, prevented an attack from another fever.

Mr. Croly said that in the City of Dublin Hospital they could not trace any case of scarlatina carried from the detached hospital into the other. Great precautions, however, were taken, and they now had an arrangement for isolating typhoid. They were very glad to give the cases of erysipelas to the physicians. If there was no danger of spreading of diseases why were detached wings built? If they had in Dublin a good arrangement for treating small-pox and cholera and a good place for convalescent patients the present hospitals should be left as they were.

Dr. Bewley said that Sir Charles Cameron must have a good deal of evidence as to where disease started in Dublin. If he had no evidence to show that the various general hospitals are the centres of infection, then his argument fell to the ground. He would like small-pox excluded from the town, as there was good evidence that it was transmissible through the air to a considerable extent. But with regard to the other fevers, unless it could be shown that there is a greatly increased prevalence of them around the hospitals, they were very well contented with the present condition.

Mr. W. Thomson said, that according to Sir Charles Cameron, there were nine centres of infection from hospitals in Dublin. If any members of the families of those present were taken ill with an infectious disease, Sir Charles Cameron would not require the individuals to be removed from their homes. Suppose there were 200 such cases through the city, and the present fever hospitals were abolished, and one outside the city was erected in their place, how would simply extinguishing the nine centres and leaving 200 centres scattered in individual houses through the city get rid of the difficulty? In a hospital the physicians, students and nurses took care, as far as possible, that they did not bring infection outside. In private cases these rules were not obeyed.

Dr. Parsons said, that if statistics proved anything, then Sir Charles Cameron had furnished very strong evidence in favour of the present system of treating infectious diseases. They were better here than in other cities where they had isolated hospitals. If the scheme was carried out they would require half-a-dozen isolation hospitals for the different diseases. How would the students who visited an infectious diseases hospital outside the town be prevented from visiting other hospitals. What held good for small-pox did not hold good for the other diseases.

Dr. Day did not agree that people were unwilling to let their friends go to a fever hospital. As regards the spread of infectious diseases, artisan dwellings had been put up within 300 yards of Cork-street hospital. They had had only one case of typhoid and one case of scarlatina from these dwellings. If students who wanted to see small-pox would come up to him, he would be very glad to show them the cases. Friends of patients were not allowed into the infectious wards, except in very exceptional cases. He had never seen a visitor coming back as a patient in the small-pox epidemic. The first small-pox case on the south side was the man who drove the small-pox van.

Dr. Falkiner said, that the management during the small-pox epidemic of conveying patients to hospital and disinfecting was absolutely perfect. He endorsed Dr. Day's opinion that patients now wish to go to a fever hospital. The poor people will go to any institution where they are properly treated.

Sir Charles Cameron replying said, he wished to make an explanation with regard to the circumstances under which the scheme was brought before the Conference of the Sanitary Authorities held in the Public Health Office. The original Con-

ference was held simply for making arrangements for future epidemics of small-pox, and also for providing one or more homes for patients convalescing from fever. There was considerable discussion at the first meeting. He was asked to meet Dr. Stafford and prepare a scheme. He was distinctly asked to include in the scheme the consideration of hospitals for other infectious diseases. Dr. Stafford fully approved of the scheme which was drawn up and was quite willing to share the responsibility. It seemed they all were agreed that two out of the three objects set forth in the scheme should be carried out. He had subordinated the third all along if the first two were accomplished. He would be glad to have the third also, because he was still convinced that what they did in other countries was right, and that it would be better to treat infectious diseases in special hospitals. The Public Health Authorities had the power of compelling all infectious cases in tenement houses to go to hospital. When he had a case of scarlatina in his own family he sent it to hospital. When his child was at the Adelaide Hospital the Resident informed him that he had got scarlatina from his child. When small-pox was received in the Adelaide Hospital there was not a house in one street at the back of the hospital which had not a small-pox case. He thought that if the Corporation of Dublin contributed £5,500 to the Dublin hospitals they might also contribute to the expense of having a hospital for small-pox. There were 220 square miles in London, and so the fever hospitals could not be established outside London, but they were in the best position that they could be. If they admitted that small-pox and typhus and scarlatina could be transmitted through the air, their position fell through altogether. He did not say that typhus and scarlatina were as contagious as small-pox. Everything done to prevent contagion must be of some use. The chances that persons in the neighbourhood of patients suffering from small-pox or scarlatina would get it were more than if they were at a distance from them. The cause of the decrease in the death-rate from infectious diseases was due to the general improvement in sanitation. He had no intention of interfering with existing Dublin hospitals. He resisted the proposal that the grants to them should be considered, but he was defeated. The scheme was proposed with the very best intentions. Having regard to the weighty opinions expressed by the leading medical men, he had decided to abandon the third part of the scheme. His paper con-

tained his honest conviction as a sanitarian. The two primary objects, namely—

(1.) The making proper provision for a future epidemic of small-pox; and

(2.) The establishment of a home for convalescent infectious cases,

he was determined to keep to, and to use all his influence to carry out.

Dr. Grimshaw, being obliged to leave, the chair was taken by Dr. Redmond, President of the State Medicine Section.

Dr. J. W. Moore, replying, said that he had heard with great gratification the statement made by Sir Charles Cameron, that he would limit his scheme to providing accommodation for small-pox patients, and of having a convalescent home for persons recovering from all infectious diseases.

TWO CASES IN WHICH UNCOMMON SYMPTOMS WERE PRESENT IN CASES OF THROAT DISEASE.

By JOHN BURGESS, F.R.C.S.

[Read in Section of Medicine, December 18, 1895.]

BEFORE entering on the subject of this paper I shall premise what I am about to read by stating that the cases to be described are not in themselves rare forms of disease, but merely uncommon developments in the course of very ordinary conditions.

I shall try to find excuse in the absence of novelty—firstly, by monopolising your time for a very few minutes and secondly, by appealing to your indulgence as a scientific Society to consider with me that peculiar form of what may be termed every-day diseases, which are infinitely more instructive in a practical sense than those highly exceptional cases which but a few of us may treat in a lifetime.

The first case is one of acute phlegmonous pharyngitis complicated by a highly unusual symptom.

CASE I.—Mrs. D., aged forty, tradesman's wife, stout, rather anæmic, awoke with a sore throat and dysphagia on the 2nd Aug., having been previously in good health. This continued until the 4th Aug., when a great gush of blood and clots came from her mouth *without any vomiting.*

I did not see her until the following morning (5th), when her throat presented the signs of a more than ordinary severe right tonsillitis, the redness extending widely from the inflamed tonsil over the soft palate. In front, corresponding to the outside of the anterior pillar of the fauces, the palate was bulged forwards, with a sense of fluctuation, beneath. On the inner surface of the tonsil there was a vertical sulcus, the remains of former disease, but there was no appearance of any bleeding point.

I opened the abscess in the ordinary way, giving exit to a quantity of sanious pus similar to what is seen in acute abscesses elsewhere. There was the usual immediate relief, and I hoped the trouble was at an end.

About six hours afterwards I was called to see her again in consequence of a second hæmorrhage—from half to a pint of fluid blood and clots—which suddenly filled her mouth. She stated she had to put her fingers in to pull forth the latter, while the fluid blood poured through her lips on to the bedclothes. By the time I saw her there was no bleeding; some thin pus was passing through the opening I had made. Corresponding to the inner border of the tonsil, where the sulcus was situated, there was an adherent clot, which extended further down the pharynx than I could see at the time. There was no history of epistaxis, nor were there signs of blood trickling along the back of the pharynx.

On the following morning, 6th August, when I visited her I found there had been no repetition of the hæmorrhage, and the hyperæmia had almost disappeared. I continued the treatment adopted—of drachm doses liq. ergot, ice to suck, and a spoonful of iced milk occasionally.

At 6 p.m. she had a third hæmorrhage corresponding in quantity (10–20 oz.) to the previous one twenty-four hours before.

When I next saw her, at 9 p.m., there was nothing to indicate bleeding, except the clot in the situation previously alluded to, which could be seen by the mirror of a laryngoscope along the right tonsil. The upper part of the pharynx was empty, and on getting her to rinse out her mouth well with cold water it came back uncoloured. The incision was exuding thin pus. On pressure applied within the mouth, and outside at the angle of the jaw, I could get no blood to proceed from the opening.

As the bleeding, for the time being at least, had ceased, I thought it wise to apply no styptics to the tonsil, but substituted $\frac{1}{100}$ gr. ergotinin hypodermically, applied ice to the neck, and fed her by nutrient enemata, forbidding anything to be taken by the mouth. Notwithstanding all these precautions another attack of hæmorrhage occurred at 1 a.m. about similar in quantity; again at 6 a.m., and at 10 a.m.—in all three, during the night and early morning. The last she thought due to washing out her mouth with cold water. I found her very collapsed with a thready pulse—140; but seeing as yet no actual indication for extreme measures, I continued the treatment by the ergotinin hypodermically,

with nutrient enemata every two hours, and the external application of ice.

She was seen that evening by Dr. Richard Hayes, who most kindly gave me his valuable assistance. As there had been no fresh bleeding, we agreed that it was best to continue the same treatment, and give a double quantity—$\frac{1}{50}$ gr. ergotinin—at 10 p.m. Subsequent to this there was no further trouble. The patient was fed by nutrient enemata for three days by myself and a nurse from St. Patrick's Home, whose extreme attention I cannot find words to praise sufficiently, and she speedily made a perfect recovery.

The interest in this case is the source of the hæmorrhage. It occurred first at midnight, 4th August, twelve hours before my opening the abscess. There had been no escape of pus anywhere, except from this incision, from which it was not possible at any time afterwards to express blood, nor was any clot seen in its vicinity.

No history of hæmorrhagic diathesis could be elicited. She had never suffered from epistaxis, nor were the gums inclined to bleed on pressure. On each occasion of the recurrence of the bleeding she was positive in her assertion that she suddenly felt her mouth full of fluid blood and clots without vomiting. With the laryngoscope I could find no more trace of bleeding than a long, thin clot attached to a sulcus of apparently old-standing disease of the tonsil. The pharynx was empty and no blood passed with the stools.

The symptoms pointed to the only possible conclusion of a leakage from a vessel of considerable size which got sealed up by a small clot, and through some movement, most likely of swallowing, this got dislodged, resulting in further bleeding. It is easy to conceive that in fits of either coughing or vomiting a vessel in an inflamed throat may give way, but in this case there was neither the one nor the other, and the spontaneous rupture of an artery in cases of this kind seems so much out of the common that in the literature on this subject I find no similar record.

A very practical point occurs. What caused the final arrest of the hæmorrhage? There were in all six attacks of from a half to a pint, of which four followed the subsidence of the inflammation. It was due, in my opinion, to a weakened cardiac systole from loss of blood, aided by the reflex contraction caused by the external application of cold. The tabloids of ergotinin were worthless.

A short time since, when reading an old book of lectures delivered at Steevens' Hospital by Sir Henry Marsh, I was reminded of a case which came under my observation, which will, I trust, prove interesting as an example of a rare complication of one of the exanthemata, and from the fact that the patient, who afterwards passed with distinction through his medical course at the Meath and Molesworth-street Hospitals, is well known to several of the Section.

Case II.—Some years ago, when living in the country, I was sent for to see a young gentleman, aged nineteen, who had suffered from what was described as a fainting fit on the previous evening after returning from a day's grouse-shooting. He was a thin muscular boy, with every appearance of health, and rather scouted the idea of being unwell. On inquiry, however, I found out that on the previous day after coming home he fainted, his face becoming dusky, and he appeared to be suffocating.

The alarming symptoms being only transitory, his people did not think it necessary to obtain medical advice, especially as their doctor was unwell. On the following morning, however, thinking it better to err on the side of caution, and their usual medical attendant being unable to come, they sent for me. The boy was up in his room, and rather indignant at any interference with his liberty. He told me he had a cold with a dry cough, which gave him no trouble, and being rather tired on the day before he felt a weakness come on when he got home.

There was no history of epilepsy. His temperature was 99°, pulse not much accelerated—the increased rhythm being principally due, I thought, to the annoyance caused by the intrusion of a stranger against his wish. The inspiratory murmur was very slightly increased over the bronchi; there was no redness of the fauces. Other than the cough there was no symptom of anything

than a very mild catarrh. I advised him to stay in his room, keep a fire there, as the evenings were cool, until he expectorated with freedom, not to speak much, and prescribed a sedative mixture.

The next morning his condition was unchanged. The cough, which was slight, continued still without expectoration. He had slight hoarseness, but otherwise no discomfort. About three hours after my visit I was suddenly called to see him by a messenger who said he had become very much worse, and on my arrival I was met by his mother with the alarming intelligence, "You are too late; my son is dead." I did not think of inquiring about particulars then, but hurrying up to his room found him lying on the floor, apparently asphyxiated. I could detect no attempt at breathing, but hoping against hope, with the aid of Dr. Penny, at present in New South Wales, his tongue was pulled well forward and artificial respiration tried. At the end of a half hour, seeing some attempt at breathing, we unfortunately tried to pour some undiluted brandy down his throat. It is possible some of it got into the larynx, for a violent spasm came on, his face became cyanotic, the inspiratory efforts sounded like steam escaping at high pressure through a small pipe, and suddenly ceased. We recommenced the artificial respiration, as we believed it alone gave a chance, with all the aids we could command—warmth to the axillæ, inside thighs and feet, hypodermics of ether, and friction—without any signs of improvement in our patient.

At length after six hours, as we were about to abandon hope, I noted the dusky hue in his face less marked, and continuing I was both surprised and overjoyed to see his eyes regard me, and hear him in a faint whisper ask for a drink of water. I remained in the house, sleeping in his room that night and the following, and on the third morning found him covered by the rash of measles.

I suspected the possibility of this from the first, both because there was an epidemic in the vicinity and that I seen a somewhat similar case while acting as Resident Surgeon at the Richmond Hospital in the year 1880—despite his mother's assurance that having had measles when a child he was not likely to get it again.

After this, with the exception of an attack of general urticaria, during which his temperature run up to 102° for a few days, he made an uninterrupted recovery, and strange to say, when years afterwards he was Resident in the Meath Hospital, he incurred a third attack, and was treated by Dr. John W. Moore, without,

however, the alarming symptoms which preceded the second onset. I should have said he was seen by Dr. Brew, of Bray, after the spasm had passed off, who was kind enough to say that the work of saving the boy's life was over before he arrived.

This case brings under your notice a morbid condition which seems strangely overlooked in modern times—as to its fatality, being generally confounded with the choking symptoms occurring in the course of acute laryngitis due to œdema.

Steffen, in von Ziemssen's work, states that catarrhs of the larynx, trachea or bronchi are not of themselves capable of producing laryngeal spasms by reflex irritation, but when the cramp, as in this case, has previously existed, they are capable of exciting it although it be disappearing, especially through paroxysms of coughing.

Laryngeal spasm is, according to all authorities, very uncommon after the first three years of life, and then almost confined to the female sex. That it is a frequent cause of death is evident from the figures given by different authorities:—

Barthez and Rilliet	-	8 deaths in	9 cases.	
Hérrard	-	6	„	7 „
Reid	-	115	„	289 „
Lorent	-	77	„	200 „
Salathé	-	2	„	24 „

In spite of this we find this condition alluded to in most text-books in a short article on laryngismus stridulus, having a usually favourable termination. The above recalls Marsh's "Published Cases," where at least if laryngitis existed, which he denied, a sudden spasm often terminates fatally what has been looked on as a trivial ailment. Here, as well as I could judge, the spasm was primary. Whatever catarrhal symptoms were present resembled more the irritable condition of the larynx described in Cohen's book than any inflammatory

change. As to the spasms in some cases being due, either, as Cohen points out, to incarceration of the free edge of the epiglottis by the aryteno-epiglottidean folds taking part in the cramp, or the partial swallowing of the tongue described by Hemming, I think this case can belong to neither, for the reason that the tongue was pulled well forwards simultaneously with the artificial respiration.

What caused the spasm I cannot say, except one can suppose that the latent zymotic disease, in the absence of its usual prodroma of naso-laryngeal catarrh was capable of exerting such an influence on the basal ganglia. No doubt tracheotomy may appear to have been indicated; yet from the condition of the patient—our uncertainty whether he was alive part of the time—made me nervous to risk any further shock. And I claim, perhaps in my ignorance, that I gained by an equally good method, and probably good luck combined, my object in saving the life of one who is now a member of our profession in large practice in England.

Dr. James Little said he had seen some cases of which Dr. Burgess' paper reminded him strongly. He thought the fact should always be kept in mind, that very severe and anomalous symptoms sometimes occur when the exanthemata (scarlet fever and measles) are about to appear. He was sure he had seen cases of this nature, in which it was impossible to make a diagnosis or to account for the violent symptoms which ushered in the illness until twenty-four hours or more had passed.

Mr. Wheeler believed that Dr. Burgess would find two cases similar to his mentioned in the *Medical Times and Gazette*. He was not sure of the date of the number containing the reports, but he believed it was one of the numbers of 1859 or 1860. As well as he could remember, one of the reports was by a Mr. Stanley. During the discussion on one of the cases a gentleman made the strange remark, that the plexus of veins external to the tonsils communicated with the internal jugular vein, and that the bleeding was from the lateral sinus. He thought bleeding in Dr. Burgess' case was from the ascending pharyngeal artery.

Dr. J. W. Moore said they had all every reason to be grateful to Dr. Burgess for having saved a most valuable life by his untiring perseverance. Dr. Burgess had communicated to him the name of the patient, who was now a medical man in England, and an ornament to his profession. Trousseau, in his Clinical Medicine, mentions a case where violent convulsions, extending over eight days, subsided on the first appearance of the rash of scarlet fever. Trousseau's case and Dr. Burgess' he regarded as belonging to the same category.

Dr. W. G. Smith believed that the explanation of the severe hæmorrhage was the division of the vertical arteries that run in the pharyngeal folds. Those vessels cannot contract as well as other arteries, hence the severity of the hæmorrhage. As to ergot for internal hæmorrhage, except that from the uterus, he considered the drug quite useless.

Mr. Woods wished to ask Dr. Burgess if he was present at the actual time hæmorrhage took place in the first case. He considered that a vessel had been opened by an ulcerative process. He was not sure that it must have been necessarily an artery. Venous bleeding he thought was a much more serious matter than arterial bleeding. The artery can contract properly, and retreat in its sheath; but this was not the case with veins, at least not to the same extent. He could not infer from Dr. Burgess' paper that he applied any local remedy. His experience was that local remedies are the best; but this did not mean that the necessity of the position of the body, so as to calm the circulation, should be neglected. He did not attach much importance to the giving of food by enemata, however good a thing it appeared at first sight. He would not say that the enemata did harm. It should be remembered that it requires a greater effort on the part of the pharynx to swallow the saliva than to swallow food. Enemata were not for this reason of such advantage, on the ground of physiological rest to the pharynx. With regard to the second case he, Mr. Woods, thought that tracheotomy might have been performed with much benefit.

Dr. Burgess, in replying, thanked the Academy for the reception given his paper. In reply to Mr. Woods he said he was not present at the actual time of the occurrence of the hæmorrhage in the first case. He arrived just after it occurred. He did not see the bleeding points, and his difficulty had he used styptics was, where to apply them. With regard to the second case he admitted that tracheotomy was the proper course to be adopted.

A CASE OF SYRINGOMYELIA.

By M. M'HUGH, M.A., M.B.;
Physician to St. Vincent's Hospital.

[Read in the Section of Medicine, December 13, 1895.]

BEFORE proceeding to an account of the clinical features of the case which I have the honour to bring under the notice of the Academy, it may be convenient that I should make a few introductory and explanatory remarks respecting the disease of which the patient will be found, I think, to furnish a remarkable example.

The term syringomyelia was first used a little more than fifty years ago to designate a condition attended with the formation of central cavities, distended with fluid, in the spinal cord. These excavations having been found to be mostly of a tubular character, the view was advanced by Virchow and Leyden that they were due, and always due, to dilatation of the central canal. They therefore proposed the term hydromyelia as more suitable to the pathological condition, and preferable to the one originally introduced. That Virchow's view was, in the main, erroneous was, however, very soon demonstrated. In most of the cases examined it became apparent that the hollowing of the cord took place independently of the central canal, which was frequently found to be occluded. The cavities were generally destitute of an epithelial lining, but were almost always surrounded by a quantity of neoplastic tissue of the nature of glioma, and it was evident that they were, in fact, produced by a liquefactive or cystic degeneration of this newly-formed gliomatous tissue. Occasionally, no doubt, the central canal became involved, but only as a

secondary result, and by extension of the process of growth and excavation into it.

This origin of the disease as a gliomatosis having been demonstrated by Simon in 1874, and confirmed by numerous other observers, the terms syringomyelia and hydromyelia were differentiated in their application. Virchow's term, hydromyelia, has been retained to describe simple dilatation of the central canal, whilst the term syringomyelia has been reserved for those special forms of cyst resulting from the softening of newly-formed gliomatous tissue.

The distinction, however, is of little practical value. The usual condition is a gliomatosis accompanied by syringomyelia occupying chiefly the cervical and upper dorsal regions. This gliomatosis depends upon a proliferation of a portion of the embryonic neurogliar tissue remaining over in that portion of the cord in which the medullary folds of the embryo close over to form the central canal, in that portion which is therefore developmentally the weakest—that is to say, the posterior part of the gray commissure and its immediate vicinity.

Gowers, in his book on diseases of the spinal cord, lays particular emphasis on the invariable congenital origin of the disease. This is, no doubt, in the main correct. Nevertheless, it would seem desirable to distinguish those cases in which there is a persistence of the embryonic tube, associated with arrested development of the cord—in other words, in which the cavity is congenital—from the more important group of cases in which the only developmental defect consists in the persistence of embryonic tissue which may, at a comparatively late period in life, undergo proliferation and produce gliomatosis. In the first group the deficiency in the cord is often accompanied by other developmental defects, such as encephalocele and absence of the cerebellum, and the subjects of it die very young.

In the second group of cases of true syringomyelia the first manifestations of the disease occur commonly in the adult. A large majority of the cases begin after the twentieth year, and many of them have arisen in persons over forty years of age.

As we have seen, the true anatomical character of the disease was recognised as early as 1874. Clinical observations were, however, at this time very much behindhand. Even in 1878, Erb, writing in Ziemssen's Encyclopædia, states that "all experience thus far teaches that the development of cavities as such produces no sort of symptoms by which it could be recognised during life," and also "it appears that we are at present possessed of no means to establish the diagnosis of syringomyelia during life."

Numerous cases of the disease were subsequently recorded in which a diagnosis of progressive muscular atrophy or of amyotrophic lateral sclerosis was made during life, and in which the true nature of the disease was only revealed on *post-mortem* examination. Schultze seems to have been the first to point to certain characteristic symptoms as diagnostic of the disease, and it is largely to his labours and writings, from 1886 to 1888, that our present clinical knowledge of the affection can be traced.

During the last six or seven years clinical observations have been rapidly accumulating, and one can hardly open a volume of transactions or of one of the medical journals without finding typical cases recorded. The first case of the kind shown in Ireland, I believe, was exhibited in the Academy in 1893 by Drs. Coleman and O'Carroll, and their paper forms an exceedingly interesting contribution to the literature of the subject.

At first sight it might seem that the formation of cavities in the cord should lead to such a diversity of symptoms as

to make an accurate diagnosis impossible. The tendency of the disease, however, to begin in the grey matter immediately behind the central canal and to extend laterally, principally into the posterior cornua, tends to produce more uniformity in the clinical manifestations than might have been expected. And our knowledge of these has now been so far perfected that we can infer the existence of syringomyelia from a patient's symptoms with almost the same facility and accuracy as posterior sclerosis of the cord in a case of locomotor ataxy.

Charcot has divided the symptoms of the disease into two classes. Intrinsic symptoms, or those more immediately related to the central lesion in the grey matter, and extrinsic, or those resulting from secondary degenerations in the white matter. Amongst the former the most striking and characteristic is what he has termed sensory dissociation—that is to say, anæsthesia for painful and thermic impressions, with retention of tactile sensibility and of the muscular sense. Remarkable trophic lesions also occur, well illustrated by the case now exhibited. Extensive muscular atrophy of the Aran-Duchenne type is also commonly seen, and as the disease usually begins in the cervical region of the cord, there is often a considerable resemblance as regards this symptom to Cruveilhier's paralysis.

The extrinsic symptoms most commonly met with are spastic, due to degeneration of the pyramidal tracts. More rarely the phenomena are ataxic. A very peculiar feature found in one half of the cases is lateral curvature of the spine.

Vaso-motor disturbances, areas of complete anæsthesia, or hyperæsthesia, and irregularity of the pupils have occasionally been noticed.

If the lesion should extend into the lumbar enlargement,

the case may be complicated by visceral lesions, such as cystitis, interference with the sphincters, &c., whilst an extension upwards may produce bulbar paralysis.

Case.—The patient, R. B., is a native of the County Mayo; by occupation a farmer, and twenty-five years of age. His family history presents no noteworthy feature, and up to his twentieth year his health seems to have been good, his only illness having been an attack of measles, which was followed by a delicacy of the respiratory organs. Between four and five years ago, however, he met with a serious accident, which consisted in a bad fall from a horse. His foot caught in the stirrup and he was dragged for a considerable distance along the ground. When picked up by his friends he was unconscious. He quickly recovered, however, from the more immediate effects of his fall, but soon afterwards noticed that his back was getting weak, and that he was not able to lift weights or to carry sacks on his shoulder as well as before. This weakness gradually increased, and about a year after the accident he began to experience pains, sometimes very acute, in his bones and joints. These were particularly noticeable in his hip-joints, and he attributes his difficulty of gait to stiffness of these joints. As time passed these pains disappeared, and the peculiar changes in his fingers (painless whitlows) became noticeable. The patient attributed them to injuries received while at work, but he observed that they showed no tendency to heal, and he therefore consulted a doctor about them. They caused him little or no pain, and he also found at this time that he could bear pain much better than other people.

I am indebted to Dr. Costello, of Ballindine, who sent me the patient in January last as an interesting clinical case, for the opportunity of examining and exhibiting him. His symptoms on admission to hospital in January were very similar to those present now, and may be briefly described as follows:—

The patient's hands exhibit trophic lesions of a symmetrical character, the terminal phalanx of the middle finger, with the soft parts, having been completely lost on the right side and very nearly so on the left, whilst many of the fingers present scars not unlike those seen in Raynaud's disease. The nails, where not partially or

completely destroyed, present transverse grooving, and the skin of the palmar surface of the fingers is marked by deep fissures, principally at the junction with the palm. Many of the phalanges present remarkable thickening, suggestive at first sight of acromegaly, and this is especially noticeable in the proximal phalanges of the middle finger of both hands. The interossei muscles show also considerable wasting. The mutilation of the digital extremities, combined with the hypertrophy of the phalanges and the wasting of the interosseous muscles gives to the hands a most peculiar and characteristic appearance.

The muscular wasting is not confined to the hands but is also marked in the deltoids and other scapular muscles and in the lower portions of the trapezii, especially on the left side. The muscles, however, respond to Faradic stimuli, though not as readily as in health. The vertebral column presents marked lateral curvature in the dorsi-lumbar region, the convexity being directed towards the right, and there is a compensatory curve in the cervical region. On examining the lower extremities the knee-jerks are found to be greatly exaggerated, while ankle clonus and rectus clonus can be readily evoked on both sides. Muscular rigidity is not demonstrable, and the gait is not very characteristic. The patient has difficulty in walking, but can cover a mile, he says, in half an hour.

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The cutaneous sensibility presents very remarkable alterations, showing typical sensory dissociation. In the hands, as elsewhere, tactile sensation is unimpaired, but when needles are driven through the skin, or when it is blistered with hot wires, no pain is experienced by the patient, who has, in fact, frequently burned himself from having unconsciously touched hot objects. I may here mention that the whitlows which produced the loss of his finger tips were almost altogether painless. This analgesia, though best marked in the upper extremities, has been found to extend over a considerable area of the body, and is accompanied with loss of heat and cold sensations—at all events within certain limits. Owing to the inexact means at our disposal I have had some difficulty in the exact delimitation of the areas of combined analgesia and thermo-anæsthesia. Both are, however, widely and symmetrically distributed, extending from the occipital region downwards to the knee-joints. All forms of sensibility are present only in the skin of the face and in that below the knees. Touch sensibility is universally present, and there is no hyperæsthesia discoverable anywhere.

The patient's vision and speech are unaffected. His eyes have been carefully examined by Mr. Odevaine, who has failed to discover any deviation from the normal in them. There is also a complete absence of ataxic symptoms, of vaso-motor disturbances and of visceral lesions.

It will be thus seen that the patient exhibits, in a remarkable way, what I may describe as the five classical symptoms of the disease—viz., sensory dissociation, painless whitlows, muscular atrophy, curvature of the spine, and spastic symptoms in the lower extremities, and a word or two of comment on some of them may not be out of place.

As regards the trophic lesions in the hands, I may remark that cases similar in this respect to the one exhibited have been observed by Morvan, of Brittany, in 1883, and described by him as Panaritium Analgicum, or the painless whitlow disease. Morvan attributed the lesions to peripheral neuritis, and in his cases there would seem to have been complete anæsthesia—*i.e.*, absence of sensibility to touch impressions, as well as to those of heat and pain. In two fatal cases, however, of Morvan's disease, cavities with gliomatosis were found in the cord at the autopsy, and with an advancing and more complete knowledge of the affection, it has now become the generally received opinion that Morvan's disease is really a type of syringomyelia in which the trophic lesions appear early and form a prominent symptom. It would therefore be correct to describe the case exhibited here as a case of syringomyelia, showing Morvan's symptom. As making the resemblance even more complete, I may mention that the peculiar thickening of the phalanges, suggesting at first sight acromegaly, has also been noticed in these cases. Charcot alludes to it, and has suggested the name cheiromegaly for it. Taken in connection with the fact that

vasomotor disturbances causing blueness of the extremities have also been observed, it is conceivable that there may be some connecting link between the disease at present under discussion and Raynaud's disease.

By far the most constant symptom in syringomyelia is sensory dissociation. It is, in fact, so invariably present that it may almost be considered to be pathognomonic of the disease. No satisfactory hypothesis has, however, as yet been offered respecting it, and in the present state of our knowledge, or want of knowledge, of the course taken by the centripetal conducting paths in the spinal cord it would be premature to attempt any explanation. Although a complete decussation of the sensory fibres probably does take place in the central nervous system, it cannot be said with certainty where this decussation takes place, or whether it takes place in the cord or not. If it should be the case that the fibres conveying painful and thermic impressions decussate in the cord whilst the tactile fibres cross higher up, the explanation of the phenomenon may lie in the fact that the gliomatosis commonly arises in or close to the grey commissure—that is to say, close to the region of decussation. As Gowers observes, the usual association of analgesia with thermo-anæsthesia makes it probable that the paths traversed by the pain and heat forms of sensibility are contiguous in the cord, and he favours the view that these paths pass by the posterior commissure to the antero-lateral tract of the opposite side. At all events, however, although the question is still in a nebulous condition, it may be confidently expected that the pathology of syringomyelia has a great future before it, and that a careful study of the ascending degenerative lesions resulting from this disease, combined with accurate clinical records, will go far to complete our knowledge of the physiology of centripetal conduction in the spinal cord.

Muscular atrophy, principally in the upper extremities, is a prominent symptom, and is sometimes so extensively distributed that many cases have been in the first instance diagnosticated as amyotrophic lateral sclerosis, or progressive muscular atrophy, and this mistaken diagnosis was, I understand, made with respect to the case exhibited in the Academy two years ago by Drs. Coleman and O'Carroll before the patient came under their observation. The atrophy is, no doubt, due to degeneration of the anterior horns, produced either by pressure of the distended cavity, or by an extension of the gliomatosis into them, or possibly by interference with their blood supply. The muscles, as a rule, are responsive to Faradic stimuli, as in my case, though not so readily as in health. Occasionally, however, reaction of degeneration is met with. Fibrillation appears to be common.

The lateral deviation of the spine is a remarkable feature, and presents the rotation peculiar to true scoliosis. It must be accepted as a characteristic symptom. According to Brühl, who has published an exhaustive summary of the cases reported previous to 1890, it is present in one half of them. Its explanation is by no means obvious, and there is plenty of scope for speculation. The most plausible theory is that the muscular atrophy, combined with a particular habitual posture, is responsible for it. As far as I know, however, it is not present in progressive muscular atrophy or in amyotrophic lateral sclerosis, at all events in early stages, and the question of its causation is therefore in need of further elucidation. Generally the deviation is towards the left side, and the case I have exhibited would seem to be exceptional, therefore, in having the convexity directed towards the right.

Contrasting the case exhibited with those heretofore described, one cannot but be struck by the remarkable

symmetry shown, in the present instance, in the distribution of the symptoms—trophic, sensory, and spastic. If one can draw any inference from this symmetry as regards the character of the lesion in the cord, it would be, I should think, that there is a single cavity symmetrically and centrally placed, with, perhaps, a lateral wing extending into each posterior cornu. As regards vertical extent, this cavity probably occupies a considerable portion of the cervical and upper dorsal regions.

In conclusion, it need scarcely be said that the prognosis is extremely grave. The disease is slowly progressive towards a fatal termination, either from hæmorrhage into the cavity, from some intercurrent visceral lesion, or possibly from the involvement of centres more directly concerned with organic life. There is also but little prospect of beneficial results from treatment. The French physicians have tried extension by the method of suspension, as in locomotor ataxy, and counter-irritation along the spine. A knowledge of the pathological condition, however, does not encourage one to persevere in the application of any of the remedial means at present at our disposal.

Dr. Craig remarked that the symptoms of the case were most classical, from the fact that they occurred in a patient from twenty to twenty-five years of age, that dissociation symptoms were so well marked, that muscular atrophy attacked the upper extremity, causing weakness; and that there was spastic paralysis of the lower limbs and curvature of the spine.

Dr. Boyd congratulated Dr. M'Hugh on having presented to the Academy a most typical example of this rare form of spinal disease. Dr. Boyd drew attention to the scoliosis which is so often a marked feature in atrophy of the muscles of the shoulders and back. In Dr. M'Hugh's case the dorso-lumbar curve principally involved the left side, in contradistinction to the usual right-

sided scoliosis in the dorsal region of growing youths and children. The pathology of the disease touched on by Dr. M'Hugh would point to the involvement of the posterior commissures of grey matter, principally due to the dilatation of the central canal. Why, under such circumstances, ataxy did not show itself as a more frequent symptom in these cases was not easy to explain. Possibly the ataxic symptoms had often been overlooked in the earlier stages of the disease; and we know that they often disappear when the lateral columns become involved in association with the posterior columns in other affections of the spine.

Mr. Swanzy said Dr. M'Hugh told them that in this case there were no ocular symptoms, and no doubt Dr. M'Hugh had investigated the matter. He would like to know if there was contraction of the field of vision, because that is no uncommon symptom in syringomyelia. But whether the contraction belonged essentially to the disease or whether it was of a hysterical character and only grafted on the disease, was not easy to decide. Some observers had found a peculiar neuritis in this affection, but he (Mr. Swanzy) thought it was not of common occurrence and it was not present in Dr. M'Hugh's case.

Dr. M'Hugh, in reply, said that the fact which Dr. Boyd emphasised—namely, that the scoliosis was to the right and not to the left side, was remarkable. The fact that the disease commonly began in the cervical region was, in his mind, a sufficient explanation of the secondary degeneration being usually descending and in the lateral columns, giving rise to spastic symptoms. A lesion beginning high up could scarcely produce a secondary degeneration of the posterior columns sufficiently extensive to lead to ataxia. The patient's eyes were examined by Mr. Odevaine of St. Vincent's Hospital.

AN UNUSUAL RASH IN ENTERIC FEVER.

By JOHN MARSHALL DAY, M.B.;
Resident Physician, Cork-street Hospital, Dublin.

[Read in the Section of Medicine, January 17, 1896.]

On page 511 of Cayley's 3rd Edition of Murchison's "Treatise on Continued Fevers," we find these words, in speaking of enteric fever:—"Occasionally when the rash is more than usually abundant, the spots are persistent, forming distinct elevated papules of a darkish-red colour, which do not disappear on pressure, and when they fade leave brown stains; the fading on pressure is a criterion which cannot be depended on when the rash is intense." And on page 515 of the same treatise, when tabulating the 12 principal points of distinction between the eruptions which occur in enteric fever and typhus fever respectively, of the former he states "a large number does not indicate danger."

I have seen several cases presenting a rash which in many points coincide with that described above, and in illustration I will describe one recent case and endeavour to point out the characters by which it may be correctly diagnosticated.

Case.—J. B., aged twenty-four years, labourer by occupation, admitted to Cork-street Hospital, October 29th, 1895. He was a healthy-looking man of average height and build, with brown hair, a native of the Co. Galway, whence he had come about 9 months ago, having obtained a situation in one of the city breweries. He gave the following history:—About 12 days before admission he awoke one morning with a headache, pain in the back of his neck, constipation, a foul tongue, and, as he expressed it, the colour of his face changed to dark yellow. He managed to work for 4 days and then took to bed; he came under our care on the 12th day of his illness, his condition on admission being as follows:—He complained of headache, want of sleep, and a great sense of weakness; the headache was chiefly frontal, but not severe; he was able

to turn in bed when asked; face pale, eyes clear, pupils slightly dilated; absence of all tremor; skin moist. On inspection we found a rash present, slightly on the forehead and face, copious on the abdomen, back, and chest; some on the thighs and arms; absent on the hands and feet. The rash might be described in the following words:—Dark red, somewhat irregular, with a tendency to circular outline, in some places like enteric spots or enteric spots in pairs, fairly well defined in outline; the spots were raised, not punctate; soft; disappeared on pressure—in fact, conveyed the sensation of enteric rash to the finger; some of them, after pressure, left an indistinct mottle, but at no time in the course of the disease did they in any respect correspond to true petechiæ. The patient stated that a few had appeared on the previous day, and more had come out the next morning—*i.e.*, the day of admission. There was a complete absence of mottling; the heart sounds were clear and regular; pulse 96; breath sounds normal; tongue dry, though moist at tip and edges, contracted if anything, not fissured; abdomen distended, but not excessive; moderate tympanites. Motions were, in the early stage of the disease, slightly fœtid, large, light brown liquid; later on they became dark brown with shreds like the *débris* of beef-tea. While in bed in hospital he had two attacks of epistaxis, which were easily controlled by the nurse. The diagnosis was enteric fever with copious rash. The patient was put on liquid diet, ordered sulphonal to counteract the sleeplessness, and given a mixture containing aromatic sulphuric acid, sweet spirit of nitre, glycerine, and carbolic acid. On the 31st of October the rash had faded, but fresh spots had made their appearance, some of which were more persistent after pressure; the intellect was still clear; said he felt better, but very weak, stated he was restless at night, the diarrhœa had increased, motions being what a nurse would describe as "typical typhoid." On the 8th day after admission—*i.e.*, 20th day of the fever—the notes were: tongue still dry, abdomen less distended, tympanitic note present, absence of pain, no chest symptoms. On the 12th day rash was fading, but few fresh spots on the trunk, brighter in colour, none on the extremities; all other symptoms improved. The evening temperature became normal on the 27th day of illness; rapid convalescence. On the 30th allowed to sit up in bed; on 32nd day of disease allowed up and put on No. 2 diet. On November 22nd—*i.e.*, 37th day of disease—given full diet; on December 6th left hospital feeling, as he said himself, better than ever he felt since he came to Dublin.

The points of interest about the rash in this case were the difficulty of diagnosis, and as to its prognostic value. I have seen at least 10 cases of enteric fever which presented this rash; the notes of some I will briefly recapitulate:—W. S., male, aged sixteen; C. S., brother, aged nine. They were both admitted from the Co. Wicklow with three other members of the same family, all suffering from enteric fever. The two boys who presented this rash were far severer cases than the others—in fact, they were a month longer in hospital. I afterwards treated the first lad for well-marked petechial typhus. H. B., male, aged fifteen; severe delirium; prolonged case; epistaxis. ——, male, aged twenty-two; severe hæmorrhage twice; recovery. ——, male, aged thirty-three; severe hæmorrhage; died. S. H., female, aged eighteen; small bit of a girl; she suffered from severe hæmorrhage during the course of her attack. T. M., male, aged thirty; died of hæmorrhage. A. D., aged twenty-one, freckled, foxy-headed young female; had a severe attack of hæmorrhage on her 21st day; died of perforation brought on by the injudicious administration of grapes by her sister on her 30th day. P. F., a Dundalk bricklayer, aged twenty-one; suffered from severe epistaxis necessitating plugging of both nostrils; severe diarrhœa; eventually recovered. T. M., aged twenty-one, medical student, male, severe diarrhœa, epistaxis, and serious pulmonary complications; recovery. In discussing these points we will first consider the question of diagnosis, and next that of prognosis. This rash may be mistaken for that of typhus fever or a prodroma of small-pox. In making a diagnosis we are guided by the history of the case, the likelihood of exposure to infection, and the appearance of the rash. As regards history, the average hospital patient cannot give a definite statement as to when he became ill—this especially applies to enteric fever. Even in typhus fever I have found they often per-

sisted in stating that they were only one or two days ill, when subsequent events proved they must have been at least a week sick. Enteric, like typhus, often commences suddenly. The patient in question may have been exposed to the contagion of typhus, so we had but one point to go on, the patient as he was when he came under observation. The points we noted as against the diagnosis of typhus were: clear intellect, with absence of marked headache or backache; absence of the muddy eye and muddy complexion of typhus; absence of mottling, with the rash two days old—a very valuable diagnostic point, and the *tout ensemble* of the rash as regards elevation, locality, where copious and where absent. It was more raised than typhus in my experience ever is; it was not more copious in the depending parts, not present on backs of wrists, dorsa of feet, inner side of thigh, and very little on the sides of the body. Typhus rash is uniform and general in distribution, seldom on the face, and more pronounced on the dependent parts. To diagnosticate it from the small-pox rash, the prodroma is most marked about the groins and axilla, and in fact is a purpura, and I have never seen it raised. Has a copious rash any bearing on prognosis? In my experience it has; for instance, A. D. had no bad symptoms for the first three weeks of her attack except the rash; she had hæmorrhage and deep ulceration. I have for long taught that the signs which make you expect and guard against hæmorrhage are clear intellect, high temperature, fast pulse, and distended belly; add to these the rash as described above, and you may look upon hæmorrhage as an almost certain complication. Four times I have seen these symptoms, and four times successfully foretold the occurrence of hæmorrhage. Even where you have the ordinary enteric rash abundant, I believe that you should look upon the case as likely to be severe, and give a guarded prognosis.

We find on analysis that 8 out of 10 of these cases had hæmorrhage, 5 had hæmorrhage from the bowel, and 3 had epistaxis; 3 of the cases died, showing a death-rate of 30 per cent. My thanks are due to Dr. Ashe for permitting me to bring this case under the notice of the Academy. Unfortunately there is little new under the sun; still of enteric fever, least of all of our exanthemata, can it be said, "*Ab uno disce omnes.*"

Dr. J. W. Moore said the case showed the necessity for a fever wing being attached to every hospital, as the patient in the first instance was admitted to a general ward in the Meath Hospital. When he first saw the case, he had no hesitation in pronouncing it one of typhus fever, but that it was not so, but enteric fever, the temperature chart now shown proved absolutely. He never saw a case more like typhus in the early stage. The case could not be left in the general ward, and had to be sent to Cork-street Fever Hospital. With regard to the case, he thought from the number of the spots, their unusually dark colour, and their irregular size, that they were those of typhus fever rather than an enteric rash.

Dr. Pollock said he had seen several cases in which there was the rose-coloured rash all over the body, together with all the other symptoms of typhoid, yet they were cases of typhus. The brain became rapidly involved, and all the cerebral symptoms manifested themselves.

The President gave some details of a case, apparently of typhus fever, but a fatal result produced by perforation proved that the case was one of typhoid. The late Dr. Kennedy believed there were some cases of mixed typhoid and typhus fever. The President mentioned a number of cases that occurred in Bishop-street, in which both rashes co-existed simultaneously in the same patient. He said that enteric fever was not so fatal 25 years ago as it is at present. He had never lost a typhoid patient in Cork-street Hospital, but this he did not attribute to his superior skill, but to the mild form the disease assumed at the time he was connected with that Institution; on the other hand, cases of typhus fever were then far more numerous.

Dr. Day briefly replied.

A FATAL CASE OF CHOREA.

By JAMES LITTLE, M.D., F.R.C.P.;
President of the Academy;
Physician, Adelaide Hospital;
And JOHN ALFRED SCOTT, M.D., F.R.C.S.;
Professor of Physiology, Royal College of Surgeons.

[Read in the Section of Medicine, January 17, 1896].

THE chief interest of the case, the details of which I am about to give, lies in the appearances Dr. Scott has so beautifully displayed under the microscope. As we all know, a fatal case of chorea is happily a rare event; but that is a reason why I was anxious to make the most I could of the present one, in the hope of elucidating the minute changes which take place in the brain in this disease. Like most cases of chorea major, and most cases of fatal chorea, the disease, in this instance, occurred in an adult. Of course, we are all of us aware that chorea usually occurs amongst children, and in them rarely proves a fatal disease. In every respect I consider that the case I am now bringing under the notice of the Academy strikingly illustrates the usual history of chorea—at all events, chorea as we see it in its most violent form. It occurred in a woman twenty-one years of age, and who had a decidedly bad family history. Her mother had been in a lunatic asylum, and other members of her family suffered from other illnesses which showed that they belonged to that class prone to nervous diseases. Her own history, too, was unfavourable. When about five years of age she got into a nervous state in consequence of a severe beating. This nervous state lasted for a long time, during which she might be described as highly excitable. Before her admission to hospital she went on a visit to friends

in the country. One night she drove a distance of eight or nine miles. The weather was cold, and she suffered as a consequence. A day or two afterwards she had pains and swellings of her joints, a circumstance which illustrates the connection between acute rheumatism and chorea. She was then brought back to town and sent to hospital. When admitted she presented all the well-known characteristics of a case of severe chorea; and though both sides were implicated, as is usually the case, one side was more affected than the other. She was quite unable to articulate, and appeared not to understand what was said to her. She swallowed with great difficulty. There was a systolic murmur audible at the apex. She lived ten days after her admission. The chart of the case shows that the temperature during the attack was always above the normal, varying from 102° to 104°. We tried several drugs, but the one that exercised the most distinct influence in weakening the severity of the twitching and spasms was hydrobromate of hyoscine, of which $\frac{1}{200}$ of a grain was injected under the skin. Still the effort to swallow became more difficult. She became exhausted, partly by reason of her inability to swallow, and partly on account of the incessant choreic movements. The details of the autopsy I leave to Dr. Scott to describe. The case illustrates most of the features characteristic of chorea—for instance, that the disease occurs in persons belonging to a neurotic family; that it has a connection with rheumatic articular affections, and not infrequently is complicated with endocarditis; that one side of the body is more affected than the other; finally, that it is in the adult it is likely to have a fatal termination.

Dr. J. Alfred Scott reported as follows:—

"At the *post-mortem* examination some very small vegetations were found on the mitral valve; otherwise the heart and viscera were normal. The brain was removed, and subsequently sections were made from various parts, but only in the motor areas were

any unusual appearances noted. In this part most of the large ganglionic cells contained a yellowish patch occupying about one-fourth of the area of the cell. Staining with osmic acid resolved these patches into granules, which were blackened by the osmic acid, but which resisted other staining agents.

"It is probable that these degenerate patches were the result of the exhaustion caused by the very excessive muscular movements rather than the cause of the disease, as similar conditions are found in other diseases associated with nervous exhaustion, and that the real cause exists in the cells before any change in structure takes place."

Dr. Pollock mentioned the case of a young woman, aged eighteen, attacked by chorea. This case recovered. The spasms were very marked. Bromide of potassium, 20 grs. twice daily, and 30 grs. at bed-time, gave some hours' rest, but the moment the drug was discontinued the spasms returned as bad as ever. He administered arsenic in minute quantities. There was endocarditis and a murmur. Joints were also affected.

Dr. Dawson said he would like to ask Dr. Scott at what stage in the hardening process he made the section. He considered the degeneration like that which commonly accompanied insanity. He believed it was due to over-action of the cells. He would like to know the nature of the staining substance used?

Dr. Boyd wanted to know if an examination of any other portion of the nervous system, except the cortex, was made; whether the ganglia at the base of the spinal cord were examined; and whether attention was paid to the condition of the capillaries.

Dr. Cox gave the details of a case of chorea. It was a child. The faculty of speech was lost completely; temperature high—104°; great wasting of muscles of both arms and body; unable to support head; food administered with great difficulty. Things looked as if the case would terminate fatally. He tried bromide of potassium, but found it of little use. What had a decidedly good effect was Easton's syrup with arsenic. The strychnin he gave, after some hesitation, in the hope of stimulating the respiratory centre, for he was alarmed lest the respiration would stop at any moment. She recovered. He believed that the spinal cord played as important a part as the brain in the production of chorea.

Dr. Pollock said he had given strychnin in chorea, but never found it of use.

Dr. Knott gave the details of a case, and pointed out what he considered the important circumstance about it was that the rheumatic symptoms followed the choreic symptoms.

Dr. Harley stated the details of some cases of chorea in a children's school that came under his notice.

Dr. Parsons said chorea in persons over twenty years of age is a rare affection. The absence of pregnancy in Dr. Little's case added to its interest. He thought the vast majority of cases would recover if left alone. Only three per cent. of ordinary cases die, whereas when the disease occurs in people over twenty, according to Dr. Gowers, who had collected a large number of cases, the mortality was 20 per cent. Death, he (Dr. Parsons) thought, was due to exhaustion. He did not believe in the embolic theory. The injection of minute particles was a recognised fact; but it was difficult to see why they should select one carotid and go to one side of the brain.

Dr. Craig gave the details of cases to show that chorea was often due to fright. In Dr. Cox's case he believed it was the arsenic did good; 5–10 minims of liq. arsenicalis thrice daily might be given to children.

Dr. Doyle dwelt on the presence of uric acid in the blood as a possible cause of chorea.

Dr. Bewley said that by gradually increasing the doses arsenic might be given with no harm, but with much good. As regards the pathology, he said they knew nothing at all about it, nor were they likely to do so. They were completely ignorant of changes that took place in the cells of the brain; yet something did take place to account for the impulse that travelled down to the muscles when thrown into action. The cells of the cortex appeared to him to be the seat of these changes.

Dr. S. M. Thompson advocated the use of chloral hydrate.

Dr. Little, in reply, said, to answer the last speaker first he might say that the first drug employed in his case was chloral hydrate, but without any good effects. After its failure he tried other remedies. Of course they all knew that the majority of cases that come into hospital suffering from chorea will get well without treatment, with treatment, or even in spite of treatment. Other cases there are that will tax the resources of the physician, and sometimes we come face to face with people with whom relief of

symptoms for even a short time can be given, and thereby, as Dr. Thompson said, turn the balance between life and death. He mentioned a case, the child of a friend of his, that came under his care 18 years ago, and in her case nothing did so well as bromide of sodium at night and arsenic thrice daily. He believed that small doses of strychnin might do good in some cases of chorea. In answer to Dr. Boyd, he said the embolic theory was not originated by Dr. Hughlings Jackson. He believed the embolic theory was now almost universally abandoned.

Dr. Scott, in reply to Dr. Boyd, said he had not examined the basic ganglia. He found no change in the capillary vessels, nor any changes in the medulla. In reply to Dr. Dawson, he said that at the time of section the brain was half hardened. He used blue-black, and also another form of blue stain.

CASE OF CHOREIC MOVEMENTS LIMITED TO RIGHT SHOULDER AND ARM.

By WALTER BERNARD, F.R.C.P., Londonderry.

[Read in the Section of Medicine, February 28, 1896.]

The case which I have brought under the observation of this Section has afforded me an opportunity of reminding you that many of us do not attach sufficient importance to slight muscular pains, cramps, and spasmodic affections, until they show signs of leading up to progressive changes, which changes, in time, grow to complete and evident disorders of movement, as the history of my present patient will illustrate.

Those engaged in general practice for several years have experienced from family and personal histories that spasms and contractions are frequently associated with the toxic agencies of gout, rheumatism, and sore throats, and are mostly due to defective elimination of such agencies, involving a disturbance of the nerve centres, and exciting cortex and spinal disturbances. The pathology of neuritis has been sent forth more than eighteen years ago, but is not, as yet, fully understood, or made sufficiently clear, and many of our cases of peripheral neuritis within the skull and spinal column are still perplexing.

To explain more fully what I advance, I have copied a few short notes from my case book, the reading of which will not occupy more than eight or ten minutes of your time :—

Case I.—In 1887 a spinster lady, aged seventy, whom I had been attending from time to time for gouty symptoms and small joint pains, was suddenly roused out of her sleep by numbness in her left arm, and severe pain in the *left* ring finger, which she discovered was flexed on the palm. An effort of the will was unable to

extend it, and she had to lift it into its normal position. She stated that this finger "shut down" at night *only* while asleep, and the *right* ring finger only during the day, while occupied in cutting flowers or bread, or manipulating buttons on her dress. Elimination with minute doses of calomel, sulphate of soda, and other gouty remedies, prolonged the intervals of the seizures to such an extent that for the next nine years, and up to the time of her death, she was but seldom troubled with these distressing and painful contractions.

Case II.—Transitory nystagmoid oscillations, and slight facial spasms, I have seen before and after pregnancy in a woman who has a sister in our asylum. They were transitory, and did not pass into puerperal mania.

Case III.—Charcot's vague expression of muscular exhaustion is borne out by a case which I saw along with Dr. Craig, of Londonderry. A gentleman beyond the middle age, very intelligent, of active business habits, with a neurotic and gouty tendency, and with too much mental strain, became prostrate, and lay listlessly and drowsily in bed. No symptoms pointed to cerebral or spinal disorders, subjectively or objectively, but blood spots appeared on his legs, and as they cleared off his perceptions became more keen. Again, the blood spots returned, and he lapsed into the same listless condition. A storm supervened somewhat akin to, but not so pronounced as, Jacksonian epilepsy. The seizure was but momentary, and as his perceptive faculties became clearer, he complained of occipital and parietal headache. Prior to the slight convulsive attack he had tender patches here and there on the scalp, two of which were well marked, one on the occiput, and one on the vertex. Symptoms of neuritis in the lower limbs followed, from which he quickly recovered. He is now convalescent.

Case IV.—Another well-marked case due to the toxic agencies of gout and rheumatism commenced with lumbago, passed into sciatica and sacro-iliac fixed pain and tenderness, associated with wasting of the limb, cramps, twitchings, numbness, and partial loss of power and sensation. The rectum and bladder became involved later on. This case, though tedious, made a good recovery.

Case V.—On the 22nd of last October a boy, aged fifteen years, was brought to me by his mother. He is an only son, has but one sister. His family history is unimportant. Up to November,

1894, he was in good health, when he was sent as a boarder to school. In the following month he had to go to bed with giddiness, headache, and vomiting of food immediately after meals. He soon got well, however, and came home for his holidays. On his return to school his illness also returned, and though he could keep well up to the other boys with his lessons, he was unable to do so in play and exercise. On getting him to walk in my room, he shuffled, turned his toes outwards, and planted them first on the ground. While making this attempt, he fell forward, the left side of his face twitched, the angle of the mouth on that side was drawn up, and the left upper lid dropped. He became pale, but did not lose consciousness. Sensation in lower limbs diminished. Knee jerks varied. Discs examined by Dr. Donaldson showed double optic neuritis, more marked on the right side than on the left. Our diagnosis was tumour of the brain. Dr. Gowers, who had him under continued supervision in hospital, wrote on the 12th of last November, "that the localising symptoms were dubious; that there was a nystagmus, loss of the left knee jerk, slight weakness of the left side of the face, bilateral weakness of the orbicularis palpebrarum, and extreme nystagmus; left sided spasm of the face, tonic, clonic, and left sided parietal and occipital headache."

Again, on the 5th of February last, Dr. Gowers wrote that "Mr. Horsley came round to his opinion that there is an infiltrated glioma of the pons." Mr. Horsley removed the medial part of the occipital bone on each side. The operation was too late to save sight, and the boy is blind, but the sight may improve when the neuritis subsides. The operation has lessened the headaches, and the vomiting has ceased, but his powers of locomotion have diminished. He returned home with his mother last week.

As brain surgery has of late been attended with so many good results, I hope the case which has been exhibited to-night will elicit discussion as to the propriety of operative measures for the relief of the persistent spasms. Its history is as follows —

Case VI.—M. MacD. is aged twenty-two years. He first consulted me on the 16th of January, 1896. He is a tall, stout, healthy young man, rather slow intellectually, but by no means feeble-minded. There is no family history of nervous diseases, except that his mother is passionate, moody, and somewhat peculiar.

He is a twin; had whooping cough and measles without any bad effects. When about fourteen years of age it was noticed at school while he was writing that there was a jerking movement about his right shoulder. On his visit to me the muscles of the right shoulder were in a constant state of movement quite independent of the will. The flexors of the fingers were also in frequent action. He is unable to put his right hand to the top of his head, to button his coat, to write, or to feed himself with his right hand. Shoulder, arm, and hand are at rest only when lifting heavy weights, pushing forward a cart, or at manual labour requiring much effort. The movements are worse on Sunday, and are increased by rest. Heat and cold make no change. Movements are not altered by closing the eyes. The field of vision is normal. When standing, he keeps his hand on a chair to steady his arm. His family say this constant movement is present during sleep. The muscles round the shoulder, arm, and fore-arm are remarkably hypertrophied, and there is some lateral curvature of the spine. He walks with his hand a little behind him, the arm being stretched out.

For the last year the friends say that the jerky movements have increased. He never had hemiplegia, and the causation is obscure. It is not secondary to disease.

Dr. W. G. Smith said the case seemed to him to belong to the choreic group of diseases.

Dr. O'Carroll said he believed the movements were simply habits acquired in boyhood, and that these habits became more marked as the patient advanced in years. To illustrate his meaning, he described some instances of habit spasms.

Dr. Drury thought the condition resembled torticollis, but without involvement of the neck muscles.

Dr. Craig considered that the disease affection in the arm was of a choreic character, and that in all probability it was the result of an infantile monoplegia that had not been recognised.

Dr. Bernard, in reply, admitted that the movements to be observed in his patient simulated chorea; but the muscles were enormously hypertrophied. The movements ceased whenever the patient had to do heavy work, such as shoving a cart, lifting heavy weights, &c. He had never seen a case of chorea in which the movements did not cease during the night.

TWO CASES OF PARAPLEGIA.

By HENRY T. BEWLEY, M.D.;
Assistant Physician to the Adelaide Hospital, Dublin.

[Read in the Medical Section, February 28, 1896.]

DURING last summer I had under my care in the Adelaide Hospital two cases of paraplegia, of which, at the request of our Secretary, I am about to give an account. I regret the notes I have of them are not more complete. I was away for my holiday during the time they were in hospital, and I trust the Section will excuse the deficiencies in my accounts:—

CASE I.—Mrs. D. came of a consumptive family. She married and had two children and no miscarriages. She enjoyed good health until the end of March or beginning of April, 1895. She then began to perceive a prickling sensation at times in her right foot. Soon she found she could not move the foot, and did not feel clearly when things touched it. The loss of power of motion and of sensation extended up the leg rapidly until she became unable to walk, not being able either to move the limb or to tell when it touched the ground.

On April 27th she was admitted into one of the Dublin hospitals. Leeches and blisters were applied to her spine. The left leg and then the left arm became paralysed. She left this hospital after about a week and came into the Adelaide Hospital on May 9th.

When I saw her she appeared to be a healthy-looking woman—perhaps, slightly anæmic. She was unable to move her legs in any way, but she could move the muscles of her body. There was no wasting of the muscles. With regard to sensation, she could feel when her legs or feet were pressed, but a very slight touch was not noticed. She could not well localise where she was touched. Sensation over her body was not lost. There was no marked line of demarcation between the areas of normal and of deficient sensation, and the position of such a zone of demarcation varied from day to day. There was no girdle pain or line of hyperæsthesia.

Both knee-jerks were exaggerated, and there was ankle clonus on both sides; plantar reflexes were present. The movements of the left arm and forearm were very feeble, and the muscles were wasted, the circumference round the biceps being 8 in. as against 8½ in. on the right side and 7½ in. round the forearm, as against 8⅜. Sensation was not impaired in the arm, and, to my surprise, I found the wasted muscles contracted with the Faradic current rather more than did those of the right arm, which was normal. She had retention of urine, a catheter being passed regularly. With the aid of enemas and purgatives her bowels were regulated; the sphincter ani seemed weak, and she occasionally passed fæces in the bed. She suffered from headaches, but otherwise there was no affection of the head. Her spinal column appeared perfectly healthy. All her viscera were sound. Her arm improved after a few weeks, and power was restored in it. Sensation gradually improved in the legs, and after about a month became practically normal. To some extent she became able to move her legs. After about six weeks she could move her knees and ankles a little, but could not attempt to stand. The legs gradually became rigid. She remained in hospital till August 17th—3½ months. Various modes of treatment were employed, both by Dr Beatty during a month, while I was in the country, and by myself, without any marked effect. For the last six weeks of her stay in hospital her condition underwent no change. During this time she was in fair general health as far downwards as her hips. Her legs were weak and stiff; no wasting; exaggerated reflexes; sensation normal; they were of but little use to her. She was unable to pass water, and it was drawn off three times in 24 hours. She was lifted out of bed into an arm chair every day and lifted back in the evening. As she was making no progress, but rather exhibiting more rigidity of the legs—evidence of degenerative changes in the cord—I sent her home in this miserable paralysed condition, having made arrangements with one of the St. Patrick's nurses to see her at her house and pass a catheter for her.

Last Sunday week a woman walked up to my house and asked to see me. She said, "Don't you remember me? I'm Mrs. D." I could hardly believe it. I examined her forthwith, and could detect practically nothing amiss with her. She told me she had been fretting much about her children while in hospital, and was happier when she went home to them. The nurse once passed a catheter for her; after that she never needed it. She continued

taking a mixture I had given her containing iron and salicylate of sodium, and the nurse rubbed her legs every day, and soon power began to return. She began to walk—a little at first, then farther—now she can walk four miles. When I examined her all the stiffness of the legs had passed off; ankle clonus had disappeared, and the knee-jerks were not excessive.

With regard to the diagnosis, I believe the case to have been one of myelitis of the dorsal region of the cord, not very severe in degree—at least not sufficiently severe to abolish sensation below the inflamed area. I think there was also some lesion of the grey matter of the left side of the cervical enlargement. Seeing, however, there was weakness and wasting in the arm, while Faradic excitability remained, I think only some of the cells in the grey matter were affected.

The point that struck me most about this case was her perfect recovery after three months of helpless paraplegia without improvement. That there had been some organic lesion of the cord was shown by the course of the disease and by the rigidity and greatly increased reflexes. Within four months all these passed away and her nervous system was restored to perfect health. I was much surprised at her so complete recovery.

Case II. was that of a young woman, a school teacher, aged twenty-nine years. Her brother had been affected for a long time with what she called "paralysis of the chief nerve of the back." She herself had, about a year or two before, suffered from loss of voice. She had consulted various doctors and numerous therapeutic measures had been employed. Among other doctors, Mr. J. H. Scott treated her, and tells me what she suffered from was undoubtedly hysterical aphonia.

In the autumn of 1894, she came to Marlboro'-street School for a year's training. About the 12th of June, 1895, Dr. Pratt, the Medical Officer of the College, was sent for as she had suddenly become unable to pass water. Stupes and other measures had no effect, and it became needful to pass a catheter. Soon after she

complained of tingling in the feet; this extended gradually upwards, and as it passed upwards, the parts first affected became anæsthetic; then she lost power in her legs.

She was admitted into the Adelaide Hospital on June 16th. Her legs were then quite motionless; she had no power in the muscles of her hips and lower part of her back, so that she was unable to turn or move herself in bed. From the mid-dorsal region downwards she could not feel any touch; the upper limit, however, of the anæsthetic area was ill-defined, the sensitive gradually merging into the insensitive portion. There was no zone of hyperæsthesia or girdle pain; reflexes were absent in the affected parts; it was needful to pass a catheter for her. Her bowels were extremely confined; calomel, Epsom salts, castor oil, and enemas, all being employed before any action occurred, and then the motion was passed in the bed; she did not seem to know of its coming. There was no feverishness; and in all other respects her organs were healthy. In fact she was a stout, rosy-faced young woman, who, as far as looks went, appeared in good health, and whose mental state was strangely cheerful or even complacent, considering the grave state she was in.

In this condition she remained for about a week, and then began gradually to improve. Unfortunately a too hot stupe was applied to her back, and thus blisters and a sore were caused, otherwise there was no trouble with her back. By the beginning of July—three weeks from the commencement of her symptoms—sensation had become fairly good everywhere. There was some tenderness, not marked, over the fifth dorsal spine, and also to a lesser extent in the lower dorsal region. Still there were no reflexes in either body, arms, or legs. By the 20th of July the functions of her bladder and rectum were fully restored. She began to be able to walk.

On August 10th she went to the Rathfarnham Convalescent Home. At that date she could feel perfectly everywhere, and she could walk fairly well. As motion returned in the legs, the reflexes returned too, and that in an exaggerated form, the knee-jerks being increased, the patellar reflex being well-marked, and there being some ankle and rectus-clonus. She was unable to retain her urine more than about two hours; if she did not then relieve her bladder, it would empty itself in spite of her; otherwise she was in very good health. She stayed two or three weeks at the Home and then went home to Co. Sligo.

I have within the last few days seen this patient. She has now no signs or symptoms whatever of her illness, except that she cannot retain her urine more than 1–2 hours by day, though she can do so for a longer time at night. The urine was healthy. I may add that though she can walk very well, she states with evident vexation that she staggers if she attempts to dance.

With regard to diagnosis, the opinions of those who saw her varied, some voting for myelitis, others for hysteria. In favour of the latter were the facts that both she and others of her family had suffered from hysterical manifestations; that the attack had commenced with retention of urine; that although the anæsthesia, paralysis, and loss of reflexes were so extensive, there were no bed sores, and also that the upper limit of the area of loss of sensation was ill-defined and rather seemed inclined to vary from day to day. On the other hand, in favour of myelitis were the passage of fæces into the bed, the complete absence of reflexes. (I am speaking of the early part of her illness.)

For my own part I told the class I thought she had myelitis; and I think the progress of the case supported this view, as when the reflexes returned, they did so in an exaggerated form, with ankle clonus, &c. However, I am not prepared to say that with the myelitis, there may not have been an hysterical element too.

Dr. W. G. Smith said Dr. Bewley was to be congratulated on his lucid exposition of two obscure and difficult cases of nervous disease. He dwelt on the terms functional and organic, in reference to nervous disease. He thought one should be cautious in accepting the precise rules laid down in text books. He gave the details of one case, in which a pathologist would expect to find a healthy cord, with perhaps islands of disease here and there. On the contrary, an autopsy revealed a cord uniformly diseased.

Dr. Falkiner thought it would be difficult to discuss Dr. Bewley's cases. For the purpose of bringing about a debate, how-

ever, he would enter into the history of a case that was under his own treatment for some time. A corn porter got a chill; twenty-four hours after he had a severe pain in his back. Soon after he lost sensation and motorial power in the lower extremities. The slightest movement of the body caused a copious flow of urine. He was sent to Baggot-street Hospital, where he was under treatment for a long time.

Dr. Craig reported a case of transverse myelitis, involving the lower dorsal and lumbar regions of the cord, in which there were, a rapid wasting, loss of motion, sensation, and reflexes in the lower limbs, with retention of urine, cystitis, and acute bedsore. Partial recovery took place, but the limbs were now contracted. The bladder had been washed out daily and every care taken to prevent the sore spreading.

Dr. Drury stated some views of Dr. Gowers' on ankle clonus. He discussed the phenomenon of ankle clonus as an aid in differentiating functional from organic diseases. He discussed Dr. Gowers' method of measuring paralysed limbs.

Dr. Bewley said, in reply, that as regarded Dr. Drury's remarks on ankle clonus, the term had for him the meaning ordinarily attached to it. He described what he meant by ankle clonus; also the manner in which the phenomenon is produced. Ankle clonus was not, in his opinion, a symptom of what was known as functional nervous diseases.

DEFECTIVE METABOLISM IN ITS RELATION TO GOUT.

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THAT the normal metabolism of the human body, both as regards tissue change and the elaboration of the alimentary substances introduced from without to maintain them, should produce no disturbing effects on the organism of a healthy individual, we regard as the first principles of physiology.

By a perfect metabolism we mean a perfect functioning of all the various physiological processes that take place in the body. For convenience of classification we group these processes under anatomical heads called systems, all interdependent on each other in maintaining the health of the individual, and derangement in any one of which is followed by imperfect functioning of the others, producing evidences of defective tissue change, both destructive and constructive—all of which the term metabolism comprehensively includes.

In this paper I am only concerned with those systems that seem to me to be mainly responsible for the production of a vicious change in this metabolism, leading to what we understand as uricacidæmia, or gout. According to the most recent views gout is uric acid in excess, which, driven from the blood in a sub-alkaline condition of that menstruum—being no longer held by it in solution—precipitates itself in back waters, so to speak, of that fluid where the stream is slowest and weakest, carrying with it in its flight some of

the soda salts of the blood; and, after precipitation, robbing the tissues around it in its new resting-place of all alkaline salts contained in them, to return again to the blood when a rising alkalinity of that fluid tempts it back, or to remain in its resting-place if a sub-alkaline condition of the blood continues. Both in the blood and out of it in the tissues, it is, when beyond a certain proportion, a foreign body, and manifests itself in both situations by well-marked symptoms. All recent writers on the subject are nearly agreed as to this being the condition of affairs in gout; but what they are not agreed on is, as to why uric acid in excess should be present in the blood?—and it is on this aspect of the question I venture to offer some remarks.

Whence comes this excess of uric acid? According to those who regard it from a merely chemical point of view, the excess is due to the food ingested being too highly nitrogenous, and the blood—being low in alkalinity from acids introduced with them—is an insufficient solvent for uric acid formed out of the compounds of urea at or in the renal barrier, and the kidneys failing to eliminate, it is carried back again into the circulation by the renal veins; whilst others explain it by the individual possessing a hereditary tendency to form uric acid in excess, and the kidneys failing in elimination, it gradually accumulates in the blood. These are the chemical views of the ætiology of gout, with the corollary deduced from them as to its rational treatment from a purely chemical point of view, of giving alkalies, or alkaline salts, that render the blood more alkaline, and are good solvents of uric acid, and behold the uric acid is eliminated by the kidneys, and appears in increased quantity in the urine. This simple view of the matter has opened the gates to all kinds of quackery and quack remedies, all claiming to be solvents of uric acid; and the professional, as well as the public, mind is captivated by the brilliant results

that have been said to follow the exhibition of such remedies. But after a little the gouty symptoms return again, and then the thoughtful mind turns to consider what other causes than mere chemical ones will explain this vicious recurrence. Nearly every writer on the subject—since Garrod discovered uric acid in the blood of the gouty—has put forward theories to account for its presence, and its over-production. Imperfect oxidation of urates, put forward by Murchison, and advocated by Garrod and later writers, certainly seemed to take hold of the professional mind, and has to-day many strong advocates. It has certainly, in theory at all events, chemistry to recommend it. Lately Harbaczewski, from chemical investigations, has brought forward the view that uric acid is the metabolic product of nuclein derived from leucocytes. Its increase in the blood and urine in leucocythæmia, and in pernicious as well as in other forms of anæmia, lends strong support to his views. All cases of gout are, however, not associated with symptoms of either leukæmia or pernicious anæmia, though a transient anæmia is very frequently associated with the first symptoms of an excess of uric acid in the blood, and one form of gout—that known as atonic gout, occurring in the flabby and lymphatic—is characterised by anæmia as a marked symptom; but to this I will allude later. Harbaczewski's theory as to the source from which this excess of uric acid is derived applies to only one of the great physiological systems—the circulatory one; but, in my opinion, defective metabolism in others exercises from time to time a preponderating influence in this excess, and it is with a view of calling attention to this that my remarks principally apply. As far as my views have led me along this line of observation and investigation, it has, I think, a very important bearing on the question of treatment of uricacidæmia and gout, for we cannot—looking at the matter from the mere chemical standpoint alone—hope to rid our patient of this

vicious tendency by merely dissolving out uric acid from his system if we leave the causes that are constantly adding to it untouched. What these causes arise from will require a review of all the various physiological processes of the body, included in the system groups I have mentioned, and defective metabolism in which adds to the blood the various compounds of urea in such quantities that the excretory organs, especially the kidneys, cannot deal with it, and in their efforts to do so form from the light soluble urea compounds the heavier and more concentrated uric acid. This defect, as we all know, increases with age as the tissue metabolism becomes less and less active, and its first onset is coincident with this lessening of metabolic activity. The views put forward by Murchison as to lithæmia being the forerunner of gout seem to me to afford strong evidence in this direction. Lithæmia is but the first evidence of lessened metabolic activity, and as age increases, this lessened activity increases with it, until such a condition is reached that one of the most simple metabolic processes in the body—the conversion of proteids and waste products into urea—becomes a most complicated one, leading to a gradual auto-intoxication, having lithæmia at one end, in its primary stage, and uric acid at the other, in its final one. That this metabolic defect is not confined to one function or one system alone I am firmly convinced, and though the digestive system is singled out by writers of to-day as the chief sinner—and in the majority of cases with very good reason—it is not the only one. I shall take up the various systems in the order of their importance, and begin with

THE NERVOUS SYSTEM

first, as it dominates and overshadows every physiological and pathological process of the body. Its influence in the production of gout is invoked by every writer on the subject

since the time of Sydenham, and in his Treatise on Gout, Sir Dyce Duckworth lays stress on the undoubted influence exercised by the nervous system in its production. Many other writers do the same, including Dr. Ralfe. Sir Dyce Duckworth regards gout as a neurosis, which descends as a hereditary legacy from progenitors. Given the same conditions in two individuals to form uric acid in excess, the individual with strongly-inherited neurotic tendency will develop gout, and the other will not. We know in other neurotic affections that the neurotic tendency is a transmitted or inherited one, and it to some extent explains how the liability to gout in families becomes hereditary, from an innate defect in nervous metabolism transmitted from one generation to another. That this neurotic element is strongly marked in gouty families we cannot deny, and that gout alternates with other neuroses in such subjects is equally well known. The descendants of a gouty stock, though they may not themselves show evidences of uricacidæmia, are the victims of other neuroses—such as epilepsy, hysteria, hypochondriasis, megrim, insanity, insomnia, and neuritis, with many other nervous affections.

Nor is this instability of the nervous system confined to those who inherit the gouty tendency, for in those who acquire it by indulgence we have evidence of this instability. Such individuals possess a nervous system that is (to use a popular expression), "easily upset;" they are of an anxious and sensitive disposition, choleric and irritable, easily cast down and easily excited by trifles, with evidences that point to vaso-motor equilibrium being easily deranged, and a tendency to flush and sweat from slight causes. Amongst women such instability is evidenced by hysteria, dissatisfaction, and emotion, morbid nervous energy, with palpitation and ovarian irritability, and, at the menopause, a tendency to involuntary flushing and tachycardia, with or without symp-

toms of Graves's disease. In fact the gouty individual possesses a vulnerable nervous system, which is not confined to one portion of that system alone, as the higher centres, as well as the vaso-motor and trophic ones, share in it.

How will these evidences of altered metabolism on the part of the nervous system produce an increase in the blood of morbid material, out of which uric acid is formed? By the increased disintegration of nervous elements, which must accompany all perverted over-action in any part of that system. We cannot originate a single mental conception, nor produce a volitional impulse, the result of that conception, without causing a change in the metabolism of the cortical cells of the brain, and this extends to all parts of the nervous tracts that take part in it. We are all familiar with the causes that directly produce or originate gouty attacks from mental conditions in the predisposed, such as over-work of brain, anxiety, exhaustion of brain and nervous system, loss of sleep, and shock from any cause. Even in many diseases of the brain, accompanied by increased disintegration of brain cells, uric acid and gout are frequent accompaniments; and our lunatic asylums furnish abundant examples of such conditions. The blood is flooded by the products of such nervous metabolism, or rather defective metabolism; and when the nervous regulating mechanism that presides over their conversion into simpler compounds to assist in their elimination is in an unstable condition, they accumulate in it.

Nor is evidence wanting on the physiological and chemical side to show this, as the following substances have been found in nerve-tissue associated with defective metabolism—xanthin, hypoxanthin, and kreatin, substances closely allied to or easily convertible into uric acid.

THE DIGESTIVE SYSTEM.

The system which comes next in importance, from my point of view, as a factor in this imperfect metabolism is

the digestive system. Indeed, it may be broadly stated that in the acquired form of gout it plays the *rôle* of chief factor, and, according to Dr. Haig, the only one in the production of uricacidæmia. We have to thank this observer for the side-lights he has thrown on the pathology of uric acid, and the patient labour he has bestowed in experiments on himself to show the dietetic causes which produce an excess of it in the blood, and for the ingenious theories he has brought forward to explain its behaviour and its various effects, both in the blood and out of it, many of which, supported by experiments, appear to be reliable. He has established and confirmed the important fact that uric acid exists in the blood in a definite ratio to urea, which in health is about 1 to 35, and that any increase of it above this proportion constitutes a condition of uricacidæmia; its future behaviour as to elimination, or increase, being altogether regulated by the alkaline or sub-alkaline reaction of the blood, and by additions from the dietetic side. He has added to and confirmed the valuable investigations into the chemistry of uric acid made by Sir A. Garrod, Sir Wm. Roberts, and Professor Latham.

Whilst I fully agree with him that the chief causes for the excess of uric acid come from the digestive side (due to faulty metabolism in the splitting up of nitrogenous compounds), I cannot agree with him in the views he holds, that the derangements in the various parts of the body (the digestive tract included) are but the results of the presence of uric acid present or precipitated in them, and not the cause of its production.

From my point of view, embarrassment of function in any part of the digestive tract, arising from dietetic excesses in the first instance, or to gradual lessening of functional activity as age advances, or to changes in the normal secretion of the glands that take part in it, the result of disease or derangement, is the cause of this defective metabolism, which

leads ultimately to the plus production of uric acid. Let me take up the first cause I have alluded to—dietetic excess—in its bearings on the subject. We all know from observation how long dietetic excesses may be indulged in by the young, and also by the healthy adult, whilst the fierce fires of a healthy metabolism are unchecked by derangement or disease. As long as the digestive organs are healthy the only result that follows the excess is an increased quantity of lithates, or urates, deposited in the urine from a temporary embarrassment, but the perfect metabolism soon returns to its normal condition, and no ill results follow, and this condition may go on for years. But let a catarrh of the stomach arise from any cause leading to fermentation in the gastric contents, and the formation of fatty acids, and then the first evidences of a defective metabolism in nitrogenous compounds commence, increased gastric acidity with lowering of blood alkalinity from their absorption follows, and uric acid begins to manifest its presence. With the disappearance of the gastric catarrh a healthy metabolism again asserts itself, urea is excreted in normal quantity, and uric acid falls, not to return again until a fresh gastric catarrh supervenes. If the individual is now nearing the period when the general metabolism is beginning to fail, and its fires to burn lower (which in many individuals takes place towards the end of the "thirties," but may be delayed to the "fifties" or even "sixties"), he realises, as the popular phrase puts it, that "he has a stomach or a liver," which may be translated into—his metabolism is failing, and uric acid is being constantly added to his blood. If from alcoholic or dietetic excesses, his stomach catarrh now becomes permanent, or his liver functions deranged from the same cause, the picture is complete. There is no longer a healthy rebound in his metabolism, his waste products accumulate, and uricacidæmia is established, and a trivial accident, or lowered nervous

influence — the result of some illness — may determine precipitation in his joints.

Though this defective metabolism may have its origin in different portions of the digestive system, I am satisfied the stomach is by far the most frequent situation of its inception. The various derangements of this organ, associated with fermentative changes, and lessened or altered secretion with dilatation, and the formation of fatty acids, and of toxins, the result of microbic growth, must all contribute in lowering its metabolic activity. It is well known to the observant physician how frequently dyspepsias arising from these causes precede for years the advent of the gouty manifestation; and he can generally foretell its ultimate appearance from their persistence. Persistent acidity is a well-known symptom in those who ultimately become gouty.

That the liver plays a most important part in the metabolism of nitrogenous compounds there can be no doubt, and that its derangement is followed by a lessening of this metabolism we must all admit.

It is not necessary I should allude to the physiological evidences in support of these facts, as the literature of the subject has established them; but I may again allude to the observations of Murchison in his Croonian Lectures on "Functional Derangements of the Liver," in which he points out that lithiasis is the result of a faulty metabolism of that organ, due to functional derangement; that this symptom occurring in children generally indicated a transmitted tendency to imperfect function on the part of the liver; and that the subjects of it were generally the children of gouty parents. I may also allude to the chemical investigations of Prof. Latham, in connection with defective metabolism on the part of the liver, where he states:—"Just as in diabetes the essential fault lies in the inability of the system, either in the liver, or it may be elsewhere, to effect the

metabolism of glucose, which then passes into the circulation, and is discharged by the kidneys, so in gout or gravel, the imperfect metabolism of glycocin, a derivative of glycocholic acid, is the primary and essential defect. Unchanged, it passes from the alimentary canal, or elsewhere, into the liver; there, under the action of the gland, it is conjugated with urea, resulting from the metabolism of the other amido bodies, leucin-tyrosin, &c., and is converted into hydantoin, or a kindred body, then passes on to the kidneys, to be combined there with another molecule of urea—forming ammonium urate—a portion of which overflows into the circulation, and is converted into sodium urate." Besides functional derangements of the liver inducing a defect in its metabolism, it is a matter of observation how frequently catarrhal congestion of that organ precedes the actual manifestation of gout, and an arrest of the secretion of bile, with clay-coloured stools, often precedes an attack. Ebstein has recorded cases of hypertrophic cirrhosis in gouty men even without portal-venous obstruction; and Murchison noted the frequency of cirrhosis in connection with gout, and remarked that "the condition of liver which develops gout renders it liable to suffer from alcohol, even in small quantity;" and Sir Dyce Duckworth observes—"The liver is the organ in which in health uric acid is chiefly formed, and it is probably to derangement of function in this gland that we must look for over-production of this substance."

Another portion of the digestive system which contributes a very considerable element in this defective metabolism is the intestine; here, as in the stomach and liver, we have constant evidences of it—associated with or preceding gout. Constipation from inanition or atonic muscular action is constantly found, alternating with fermentation of its contents and diarrhœa, and a proneness to catarrh from slight causes.

The researches of Bouchard in his work, "Auto-intoxication,"

have abundantly proved the many toxins which may be absorbed from the intestine. Most of these toxins, the result of microbes, many of which are normal inhabitants of the intestine, but, owing to lessened metabolic activity on the part of the excretory glands (especially the liver), obtain a temporary mastery, and paralyse the intestinal functions. He has also pointed out that the absorbents, from undue delay associated with constipation, abstract many poisonous compounds from the fæces which are hurtful to the economy in their passage through the blood before being excreted by the kidney. His experiments with intestinal antiseptics, and their effects in diminishing the poisonous alkaloids subsequently extracted from the urine, prove this. Kreatin, leucin, and tyrocin, all easily convertible into uric acid, are derived from the intestine during defective metabolism, as Latham has pointed out.

THE CIRCULATORY SYSTEM.

The system which ranks next in importance, after the digestive one, in the production of this imperfect metabolism, is the circulatory system; and, if Harbaczewski's conclusions are correct, it should prove the most important one in the plus production of uric acid in the economy. According to this observer, uric acid is the metabolic product of nuclein derived from leucocytes, and he contends that gout is a leucolysis—a perverted metabolism in blood-formation—and he reasons from the plus production of this acid present in the blood and urine in both leukæmia and pernicious anæmia, a well-known clinical feature in both these diseases. If so, this perverted metabolism is closely connected with both blood-destruction and blood-formation, and the presence of this acid in excess in the spleen and liver in uricacidæmia is to some extent accounted for.

If an increased formation and destruction of leucocytes in

this affection takes place, it should certainly diminish their contributory aid to general blood-formation, and lead to a poverty in red corpuscles as well, and so explain the anæmia that marks many of the atonic forms of gout. But there are other portions of the circulatory system which afford evidences of this metabolic defect besides the blood. We find clinical evidences of it through the entire circulatory channels from the heart to the capillaries. Coincident with the first evidences of failure in the general metabolism in adults nearing middle age, comes an atonic condition of the heart muscle itself—its contractions have lost their strong muscular element on auscultation, and the heart becomes more excitable and more easily disturbed, more rapid after exertion, and prone to intermit, and there is a greater tendency to dyspnœa after violent exercise on the part of the individual than formerly. Pathologically there may be found no change in the cardiac muscle, but clinically we recognise that there is some defect evident, either a neuropathic or a nutritive one, affecting its muscular tone. The arteries, and especially the veins and capillaries, seem to share this, and their regulating vaso-motor mechanism seems also more easily disturbed, and their trophic functions to suffer, rendering the coats of the vessels more prone to changes—whilst the arteries from their greater resistance show at first least evidences of this, the veins and capillaries show early signs of it. Defective metabolism in the capillary circulation may, to some extent, explain the trophic changes in the skin that are so frequent in the gouty, and also the failure in elimination of uric acid by the capillaries of the kidneys, to which Garrod and so many other writers allude. Evidences are visible to the naked eye in the colour of the skin when this failure commences—as the florid man begins to look pale or livid, and the pale man sallow or bilious-looking—whilst a tendency to thrombosis occurs in the capillary veins, render-

ing them prominent. We know how frequently phlebitis ultimately supervenes in the veins of the extremities, preceded by or accompanying thrombosis in the veins of the rectum, leading to hæmorrhoids; and also in the veins of the pharynx and larynx, leading to congestion of their mucous membranes.

Dr. Haig would explain the vascular changes by the high arterial tension and contracted capillaries resulting from the presence of uric acid in the circulating blood, and the irritating and poisonous effects it exercises on the coats of the vessels and on their vaso-motor nervous supply. I fully agree with him that in pronounced uricacidæmia the sphygmograph indicates this high arterial tension, but this is quite consistent with a lowered metabolism in the circulatory system generally.

THE LYMPHATIC SYSTEM.

Intimately associated with this lessened metabolic activity in the circulatory system comes a lessened metabolism in the lymph channels, which they subtend, and especially in the lymph spaces, with lessened oxidation of the waste products contained in them, derived from the metabolism of the tissues generally. In this system we have a fertile source of production for the early progenitors of uric acid, if the heart and circulation is defective. The lymph corpuscles supply abundance of nuclein, which, according to Harbaczewski, is one of the chief sources of uric acid, and the plasma is found to be rich in leucin and xanthin. Lauder Brunton has pointed out how essential a free movement of the lymph in the lymph channels is to oxidation and metabolism, and what an influence massage and muscular movement exercises over it from the pumping action produced in the lymph-spaces by both forms of exercise. Muscular movements and respiration are almost the only means by which the circulation is

carried on in this system, as it has no motor mechanism if we except the vis-à-tergo of the heart and arteries, and if these latter are in default, delay and defective metabolism must result if muscular exercise is not taken. From the physiological side, this is an argument in proof of the fact that a sedentary life and deficient exercise conduce to uric-acidæmia and gout.

THE RESPIRATORY SYSTEM.

I will now allude to the part played by the respiratory system in this defective metabolism. The chief function of the respiratory system is to supply oxygen to the blood, which in its turn supplies it to the tissues, so aiding in metabolism, and in the combustion of waste products contributing to animal heat; at the same time getting rid of CO_2 (a waste product) and of water. If from any cause a defect arises in this system, the process of oxidation is interfered with, CO_2 is retained, and waste products accumulate in the blood and tissues generally. Have we any evidence that defects arise in the respiratory system, and that their effects lead to uricacidæmia and gout? I will take up the evidences afforded, both from the physiological as well as from the clinical side. We know those who live an indoor life, or dwell in bad air, are more prone to form uric acid than those who live out of doors or in pure air. The former get less O into their blood and in addition inhale more CO_2 (a great portion of which is retained), whilst with the latter oxidation is more perfect, and there is no uricacidæmia, though they may have the same causes, either dietetically or by hereditary tendency, for its production. Or, again, we find there is in some individuals a tendency to greater sensitiveness of the respiratory tract than in others—they catch colds easily, are more sensitive to changes of temperature, prone to laryngeal and bronchial catarrh, or to asthma and emphysema, and

these are just the individuals who ultimately become gouty. So frequently is this associated tendency noticed that many pathologists have ascribed gout to the toxins formed by a catarrhal bacillus (not yet discovered) that is supposed to produce laryngeal, pharyngeal, and bronchial catarrh. I think we can explain this liability to catarrhs to an innate defect in respiratory metabolism on the part of the individual, which is only another evidence of this general metabolic defect, here as elsewhere, though the tendency with many writers is to ascribe the catarrh to the uric acid, and to the precipitation of this peccant material in the affected parts. I am far from denying that we do not occasionally see an attack of bronchitis in the gouty, which is directly due to gouty deposit in the mucous membrane of the bronchi. As uratic deposit has been found there pathologically by competent observers, its presence here would seem to be only accidental, or only part of a general tophaceous deposition, as we know the bronchi are not a channel by which uric acid is eliminated, though it is so for other toxins that directly produce bronchitis-uræmia for instance—and, according to Bouchard, the fatty acids which indicate their presence when being eliminated by this channel, through the peculiar odour of the expired air.

THE MUSCULAR SYSTEM.

The last system I shall refer to in connection with this subject is the muscular system; and here also evidences are not wanting to show the amount of waste products they contributed to the blood both during their healthy metabolism, but more especially when this is defective. Here we have one of the largest factors in the plus production of the urea-forming compounds, which during active exercise is constantly poured into their lymph spaces, and from there into the blood. A glance at the many products of muscle

waste will show this—kreatin and kreatinin, xanthin, and hypoxanthin and uric acid. During exertion the muscle becomes acid, and many acids are developed in it—paralactic, lactic, and phosphoric acids—and, after violent muscular exercise, many of these are retained in the blood, and contribute to raise its acidity, and lessen its solvent action on uric acid. This latter acid was found to be increased in the blood by Handfield Jones, in the person of an Alpine climber, on whom he made experiments after a severe day's mountaineering; but it was found to be afterwards eliminated during the subsequent rest, but not for some time. During muscular exertion very active oxidation of the waste products takes place, O being taken in and CO_2 being given off. But, with violent exercise, the O does not seem to be added in proportion to CO_2, and the latter accumulates in the muscles, giving rise to a sense of fatigue, and lessening their metabolism,—a fact which should not be lost sight of in ordering exercise for the gouty. Moderate exercise increases metabolism, but violent exercise diminishes it.

I do not wish to finish this subject without a word as to the reasons which seem to me to favour precipitation of uric acid in the joints, so producing the symptoms of gout, as an explanation of this epiphenomenon should necessarily follow the foregoing remarks. This selection of the joints by uric acid, under certain conditions of the blood, seems to be due to lessened nutritive changes in them, and is not a mere coincidence; here, also, it seems to me, a lessened nutritive metabolism, or an atrophic condition, renders them vulnerable; in this process the conditions of their trophic nervous supply must play an important part. If the nervous system is to be especially invoked in gout, it is in the production of this vulnerability in these situations. The facts that have been accumulating since Charcot first called attention to the disease which bears his name, and to the

intimate relation he proved to exist between the joint lesions and the spinal cord, through the influence of its trophic nerves, go to show that there is a defect in their nutritive metabolism in such lesions.

Nor are evidences wanting from the clinical side to show that in this precipitation of uric acid in the joints, the nervous system is upset, and its influence in preserving them impaired. We know an individual may go on forming or retaining uric acid in the blood for years, and no gout results until some depressing influence upsets his nervous equilibrium, and then gout becomes manifest. It may be owing to a nervous shock, or to over-anxiety, or loss in business or reputation, or to an illness, or to an accident, but that it manifests itself under such circumstances we have abundant examples to prove. We all know how an accident to a joint will produce gouty pains in it afterwards, by interfering with the nutritive metabolism of its delicate structure, and it is often the first indication to us and to the patient that he is gouty—a fact which neither the patient nor the physician previously suspected.

In conclusion, the inferences we must draw from this review of the subject is, that the gouty individual is a susceptible individual, whose general metabolism is easily upset, and that this vulnerability may arise from a disturbing influence in any of the great physiological systems I have enumerated—in the nervous, the digestive, and the circulatory systems especially—and to a lesser extent in the others I have mentioned, and no treatment can be successful which does not take note of this. We should earnestly inquire into and, by investigation, ascertain in which of these physiological systems the defect first arises—a matter of no small difficulty in many cases—as, owing to their interdependence on one another, defective metabolism in one may influence that in all the others (as I alluded to in the first part of my paper).

It may be almost impossible in the general confusion to single out the ringleader in this "vice of nutrition," and in doing so we should bear Ebstein's remark in mind, that the "gouty individual forms uric acid in perverse localities, even in bones." One fact we should bear in mind, that clearing the blood and system of uric acid by alkalies, or salicylates, leaves the principal part of our work undone—namely, the correcting of the faulty metabolism in whatever system it first arises by suitable remedies directed to them. What these remedies should be does not enter into the scope of this paper, but we know we can influence their metabolism for good by many means—medicinal, hygienic, and dietetic. We must also remember we are dealing with a complex problem, which is mainly a biological one, and that chemistry alone will not solve it for us, though it has done a great deal towards it. Whatever additional knowledge we are likely to gain—and much is still needed—must come from workers in the field of biological investigation, and it seems to me it is in this direction that investigators should turn their attention for its complete solution.

Dr. N. Falkiner said that Dr. Boyd had twice mentioned that urea was derived from uric acid. He wished to know if that was correct.

Dr. Walter Smith agreed with Dr. Boyd in considering gout as a deeply-seated perversion of the nutrition of the body. The pathology of gout was still obscure. The best summary, both pathological and chemical, is that by von Neuron. Gout is attended with certain remarkable tissue changes. It is coincident in many cases, but not in all, with the deposition of sodium biurate in certain tissues, especially connective tissue. It was not known what led to the apparent or real accumulation of uric acid in gout. It was very difficult in a gouty person to exclude the possible condition of chronic nephritis, to which it often leads. The amount of uric acid varied within wide limits both in health and disease, and the causes of its variations were not known. The assumption

that there is an increase of uric acid in a gouty person rests upon six experiments. Five of these were made many years ago by a faulty process. He considered that the variations in the amount of uric acid excreted in gout varied within the same limits as they do in a healthy person, and there was no certain knowledge of the influence of gout on the excretion of uric acid. There was no evidence to prove that uric acid is a poison analogous to an alkaloid. The theory of gout and uric acid is always linked with a diminished alkalescence of the blood. He agreed with Sir William Roberts in this belief that an acid dyscrasia in gout rests upon a pure assumption. There is very little proof that the alkalinity of the blood could be altered for more than a few moments, for the kidneys keep the blood at a steady average. It is impossible that neutral urates can circulate in the body. Therefore, Ebstein's theory falls to the ground. Sir William Roberts thinks that alkalies which occupy such a prominent position in the treatment of gout, are absolutely without influence on the course of the disease. The introduction of sodium salts into the blood is dangerous with gouty subjects, therefore harm may be done by sending patients to mineral springs with sodium salts. Considering the long history of gout, the influence of heredity, of renal disease and tissue change, he thought we should be very slow before drawing any conclusion as to the efficacy of therapeutical means in gout.

Dr. Boyd, replying, said he did not mention that urea was formed from uric acid, but that uric acid could be formed from urea. He agreed with Dr. Smith in almost everything. He thought there was a condition of uricacidæmia. Lowering remedies, as bleeding, purging, and giving large quantities of the sulphates of magnesia and soda, were essentially mischievous in gout, but tonics, alkalies, and dietetic treatment relieved a great many of the symptoms which were ascribed to uricacidæmia.

AN OUTBREAK OF ACUTE (TROPICAL ?) DYSENTERY IN A FAMILY.

By J. MAGEE FINNY, M.D. Univ. Dubl.;
Past President, Royal College of Physicians;
Physician to Sir P. Dun's Hospital.

[Read in the Medical Section, March 27, 1896.]

A family, consisting of seven persons, returned to their home on 10th September, 1889, in apparently perfect health, after spending the summer at the seaside in Bray, Co. Wicklow. The family consisted of a gentleman and his wife, his mother, aged seventy, and his four children, aged sixteen, fourteen, and thirteen years, and eleven months :—

Case I.—On 11th September, Mrs. A., the old lady, was in good health, and spent the day in the Park. On the 12th, two days after her return home, she had some diarrhœa, and for this she took the ordinary domestic remedies, but to no effect. The looseness was soon accompanied by pain in the lower part of the abdomen, and continued unabated through the night as well as the following day, and on 14th I saw her. I found her with a fairly clean tongue; the abdomen was not distended, but there was pain on pressure over the large bowel in the epigastrium and the left hypochondrium. As all the motions were passed in the adjoining closet, their nature could not be ascertained at first, but when seen in the chamber and night-chair on 15th, they were found to consist of red bloody mucus without a trace of fæces, or fæculent odour, while the quantity was not more than would fill an egg cup. Nearly every half-hour or twenty minutes the calls of the bowel were imperative; there was no straining or burning pain in the anus or rectum, and the moment the small ejecta were got rid of the patient was quite relieved till the next call. The pain across the abdomen immediately preceded the evacuation. A most notable diminution in the quantity of water passed called for comment, as the total for twelve hours did not exceed a wineglassful. Rectal examination and the

introduction of the long tube gave negative results as to the presence of any intussusception, ulceration or growth, but the bowel was tender and felt rough to the touch above the sphincter. All enemata were returned unchanged.

From the day the bloody mucus and blood-stained fluid were discerned, every evacuation was seen by me, and in none from 16th to day of her death, on 25th, did a particle of fæces make its appearance; and castor oil, which was administered on three occasions, apparently never passed through. All the ordinary astringents—chalk, opium, catechu, failed to make the least impression on the number and frequency of the motions, or on their character, except that more blood-stained mucus and meat-stained water always followed the administration of oil. Three suppositories of morphin, 2 injections of laudanum, and 4 pills each of ½ gr. ext. opii. aquos. in 24 hours, failed to stop the diarrhœa or to change its character. They only reduced the number of stools from three to four an hour to one or two in that time. Twenty grains of pulv. ipecac. led to vomiting and prostration, but had no effect on the bowels. Diet, either entirely liquid and animal for three days, or entirely milk, boiled and peptonised, mixed with carrara water, and thin arrowroot (as suggested by Dr. Gordon, who saw her with me on 20th) for a similar time made no difference. At the end of first week the patient showed signs of prostration, and these became more marked three days before death, as food in any form was not taken except in very small quantities. The pain, at first a most prominent symptom, was rarely complained of "so long as she lay quiet," and the abdomen underwent no external alteration; there was very little flatulence, and no tumour or swelling were detected, and no intestinal movements noticed.

The discharges varied a little in quantity at times, and in nature—(1) blood-stained water, (2) slimy red blood, (3) pink, raspberry-coloured clots, (4) small black clots, (5) shaggy shreds of fleshy masses, half an inch long. At times when the ejecta seemed less blood-stained, perhaps the very next motion would be as red and scanty as at the beginning. The last three days and nights the motions were about 20 in 24 hours. Occasionally, though very rarely, 2 hours would elapse without one, and then again 5 or 6 would take place in an hour, and during the 36 hours preceding death, which occurred by exhaustion and failure of the circulation, on September 25th, 1889, the 15th day, the motions ran from the anus, as a thin liquid which stained the napkins a

pinkish red colour, and was of a peculiarly nauseating odour, as of decomposing meat. There was very little hiccough and no vomiting.

Case II.—Mrs. B., aged thirty-seven, was nursing her child up to the day I first visited her mother-in-law (Case I.), on 14th September, 1889; that evening she had felt faint and weak. I made her wean the baby, at least during the night, and gave her a tonic mixture of iron, phosphoric acid and quinine for two days. On 16th she had diarrhœa, and the motions were reported to be dark. This at first seemed to be due to the medicine taken, but on 17th their characters were entirely dysenteric, eight to ten in an hour, accompanied and preceded by great pain across hypogastrium and up the left side of abdomen, while the voided matter consisted of small quantities of bloody slime and shreds of mucus.

From 17th to 23rd no fæculent matter passed, the characters of the stools being completely similar to those of the other persons attacked. On 23rd, two clay-coloured motions were passed with the greatest agony, and a sensation as if the whole inside were torn and dragged, and as if the womb had fallen down. Digital examination settled this point in the negative, and the probability was that the rectal and colonic ulcers caused the pain in the passage and expulsion of the soft fæculent masses. During the night another and larger mass passed, but without so much pain, while the bloody and mucous character was much less.

Improvement steadily progressed from this time, and in the 48 hours subsequently there were but six motions, with very much less pain in the bowels and abdomen, while the character of the discharges was more and more fæculent and contained less and less mucus and blood. The urinary secretion was more plentiful, and was passed by itself. On 27th September the patient was convalescent, though very weak and prostrate, and was put on a tonic of tinctura ferri pernitratis.

The course of this case was marked by moderate febrile disturbance, the temperature ranging from 99° to 102·5°, and the pulse 92 to 112. The tongue was red and sore and often dry, but never brown or heavily furred. All through a very unpleasant taste was complained of. Thirst was very distressing for five or six days, while chilliness, and, later on, profuse sweating, marked the febrile character of the illness.

Case III.—An infant, aged eleven months; previous to this attack, healthy; was nursed at night by her mother, and by day took bread and milk. Was weaned on 15th, and as the upper incisors were pressing and there seemed much irritation in the gums, the latter were lanced on 16th. Restlessness and green stools were now present, but under Mellin's food and small doses of bismuth and pulv. cretæ arom. c̄ opio, there was general improvement. On 17th and 18th, the child slept well, and the character of the motions changed to a natural yellow colour. On the 19th and 20th the child became very restless, and had several attacks of griping pains, after which it looked haggard and exhausted, while the extremities became cold. The motions, which were up to this date fairly copious, now became most scanty, and consisted of an eggspoonful of gummy material like unboiled white of egg or bloody mucus, and for the last 24 hours no water was passed. Nothing gave real relief. A warm sitz bath, chlorodyne in minim doses and the chalk and opium powder, were but slight palliatives; the paroxysms of internal pain, which caused the child to scream out at first, became more frequent, and the little patient sank, worn out and in a semi-convulsed state, on the morning of 22nd—the 3rd or 4th day of the dysentery. The old nurse, who for 30 years had attended infants from their birth, and who was accustomed to infantile diarrhœa during dentition and weaning, expressed astonishment at the features of the illness of the last three days, and the almost complete cessation of all true evacuations for the 48 hours before death. She had never met with a similar case.

There can be little doubt that the child became infected when just recovering from an attack of teething diarrhœa, and died of the exhaustion of pain and loss of sleep. The possibility of intussusception was borne in mind, but the absence of any tumour and of vomiting excluded it.

Case IV.—Mr. B., aged forty-nine, husband of Case II, complained of being attacked like his wife on 19th. He was constantly in the sick rooms of Cases I. and II., and had no time for rest or care of himself. His symptoms were—(*a*) frequent calls to relieve the bowel—as many as ten or twelve in an hour, accompanied by pain across the stomach above pubes, (*b*) *oliguria*, a wineglassful being the total for 14 hours! (*c*) the motions of 24 hours altogether would not fill a small tea cup, (*d*) they consisted of a raspberry red fluid and glairy mucus, and all fæces were quite absent from them from

19th to 24th inclusive. There was an absence of constitutional disturbance beyond a great and insatiable thirst, a clammy nasty taste on the tongue, although that organ was clean and moist all through. The pulse range was generally between 80 and 90, and there was no fever. Owing to his anxiety about his wife and child he took little or no rest until after the child's death on the 22nd, when he was induced to rest on a sofa by day.

The treatment at first was 20 gr. dose of pulv. ipecac. given in a little syrup of orange. This, however, caused intense sickness, and during the vomiting some small-formed motions passed from the bowels. In eight hours subsequently 8 grs. more were taken, but no marked change took place in the dejecta or the number of calls. He was then ordered a pill of gr. v. pulv. ipecac. co. every 4th hour, but this was not taken with any regularity up to the 22nd. The urgency and frequency of the calls were diminished; occasionally 2 hours would pass without a motion, and then again 5 or 6 would occur in the space of half an hour. Sleep at night on 23rd and 24th was secured by an enema of 30 minims of laudanum. On the 25th, the thirst had greatly abated. and the motions (reduced in number to 3 in 12 hours) were slightly fæculent and less bloody or mucous, while the kidneys were acting better, 10 oz. being passed in the night. September 27th, solid fæces were passed in the morning after the first night of quiet, natural sleep he had had since taken ill. Their passage caused a good deal of pain in the rectum, but they were unaccompanied by mucus or blood. The kidneys acted freely. The patient was now convalescent and allowed a small quantity of animal food, such as a beaten-up egg.

Case V.—Master W., aged twelve, who, with his brother, had been sent out of the house on 19th, when his mother and father became ill, returned on 22nd, having had "diarrhœa" on the night of 21st. Examination of the stools revealed the same state of things as in the case of all the rest—viz., no fæces and but teaspoonfuls of blood, bloody mucus, or red brown slime. The course was similar—viz., at first every few minutes he had to go to the night-chair, and so urgently that often he failed to reach it in time. Partial suppression of urine. Some febrile disturbance. Temperature, 100° to 101·2°. Pulse, 100 to 110. Forehead hot and cheeks flushed; great thirst; tendency to vomit, some abdominal pain, but no tympany or tumefaction of the belly. On the 24th and 25th he slept almost incessantly, and was moved but three times in

the night. On the 27th he had 2 teaspoonfuls of castor oil and 1 of glycerine, and in four hours he passed a good motion of formed fæces, free from blood and mucus.

It is to be noted that for seven days no fæculent discharges passed from the bowels. He was allowed up. The treatment consisted in a milk diet, with bread and rice and arrowroot, and an astringent mixture containing salicylate of sodium, glyc. of carbolic acid, chalk and opium. His illness was milder as regards the pain and amount of bloody mucous dejecta than that of the other members attacked, and the fire of the disease seemed to burn itself out after the first four days, so that much less medicine was requisite, and his recovery was more prompt and perfect.

TABLE.

	Age	Onset	Termination	Duration	Result
Case I.	70	Sept. 12th	Sept. 25th	14 days	Death
Case II.	37	Sept. 14th	Sept. 27th	14 days	Recovery
Case III.	11 mo.	Sept. 19th	Sept. 22nd	4 days	Death
Case IV.	48	Sept. 19th	Sept. 27th	9 days	Recovery
Case V.	12	Sept. 21st	Sept. 27th	7 days	Recovery

Remarks.—The clinical features of the above cases showed a remarkable family resemblance, and each may be taken as a true type of the others, having regard to the age of the patient and the severity of the attack. These features were—(1) the abdominal pain, (2) the frequency of the action of the bowels, (3) the small quantity of the total discharges in 24 hours, (4) the mucus and blood of which the motions consisted, (5) the complete occlusion of the bowels to the passage of fæces of any kind, (6) the prostration and debility without fever or very much general constitutional disturbance, and (7) the oliguria and temporary anuria. To these may be added the sudden onset of the attack in each individual without any premonitory diarrhœa—

a feature true of all the cases, not even excepting the baby, who had had the green stools of catarrhal enteritis, so common to patients of that age, at the time of teething and of being weaned, and whose stools were becoming natural milk-cure motions the day before it was attacked with dysentery.

Case I.—the first attacked—was said to have been suffering from diarrhœa before I was consulted, but it is more than doubtful if this were the case, as the subsequent course of events warranted one to believe the frequency of the calls to go to the closet was mistaken for loose fæculent evacuations. A symptom of acute dysentery, which is usually one of the most pronounced and constant and most characteristic (Woodward), was remarkable for its almost complete absence in this sriees of cases—and this was "tenesmus." This is variously described as "straining at stool," "a burning feeling in the lower bowel," "a feeling that something more has to be voided"—so that the patient may sit on the closet for half an hour at a time. Possibly the comparative mildness of the attack may explain its absence, but it may also be referable to the inflammatory changes being in the large bowel rather above than in the rectum.

The absence of all fæces from the motions from the hour of the invasion of the disease, which was particularly defined in the case of Mr. B. and his son, is, I consider, the most remarkable of the features of this group of cases—as in any cases of dysentery which I had previously seen, either fluid fæcal matter or scybalous masses were occasionally voided as well as the mucus. I see, however, that Hilton Fagge states that the most careful recent observers are agreed that fæcal matter is comparatively seldom seen. Thus the paradoxical expression of Stoll is justified, when he described dysentery in the terms *morbus alvum occludens*.

The treatment adopted was dietetic and medicinal—milk,

and milky foods with thin arrowroot, formed the basis of the diet, while counter-irritation to the abdomen by rubefacient epithems, opiate injections, and astringent medicines, formed the line of medicinal treatment. Ipecacuanha was given in 20 gr. doses to two out of the five, but it produced such vomiting and depression that it was not pressed, and I cannot say, therefore, whether good might have been expected of it. I think I might have been more successful in its administration had I given it in pills, or, as now made, in compressed tabloids, and not in syr. aurant., as suggested by H. Fagge.

The last point of interest—but by no means the least—was the question of the ætiology of the outbreak, whereby five out of seven members of a family were attacked.

I regret to say I cannot satisfy myself on this point. The whole family had just returned from Bray, seemingly in the best of health. The dwelling-house was part of a business warehouse, and was occupied by several clerks, and three or four domestics, and yet the strictest inquiry failed to trace any illness of any kind, either before, during, or subsequent to this endemic outbreak. I made inquiry of the Registrars of deaths of the district, and they told me that there were no other deaths from dysentery recorded during September except the two which I had registered, and there was no epidemic of diarrhœa at the time. In 1886 and 1887 an endemic outbreak took place in the Richmond Lunatic Asylum, which was recorded in the Transactions of the Royal Academy in 1888, Vol. VI., by Dr. Conolly Norman, who attributed it to the insanitary condition of the soil and the drainage, but it died out before 1889. The only clue at all discoverable was that a friend in the neighbourhood, who had a cow, supplied the family with milk for the two days preceding my being called in, and that soon after Case I. was laid up, the child of this neighbour died of intestinal

disease. Then a query arises. Did all the five cases contract the disease from the milk, or was it not by *contagium*? In Cases III., IV. and V. I think it was so. My cases do not add to our knowledge as to treatment very much, but the ipecacuanha did not seem a specific in the two cases I employed it. Without any intention to make little of the treatment of acute dysentery by ipecacuanha—a treatment which has in India and elsewhere acted as a specific antidote, and which was found immediately curative in a case published in the *Hospital Gazette*, 1868—it may be of some interest to note the most recent therapeutic hints which I have come across in the management of this disease.

Mr. William Arthur Dickson, F.R.C.S.I., Government Medical Officer, British Guiana Medical Service, supplies to the *B. G. M. Annual*, pp. 28, 29, a note of 183 consecutive cases of dysentery without a death treated at the Plantation Albion Hospital in 1893. The ages ranged from five months old to fifty-six, the greatest number—159—being between twenty and forty. The disease under treatment lasted 7 days in 97 and 14 days in 52.

The treatment consisted of castor-oil and chlorodyne for griping, and then the ordinary astringents—catechu, kino, opium, and mist. cretæ, every fourth hour, and in two days, if not markedly improved, pil. plumb. c. op. B. P. every third hour was substituted.

"After giving the ipecacuanha treatment several trials," he writes, "I have finally abandoned it as useless."

The diet he adopted was the same I found useful—viz., milk with bread softened in it, sago and arrowroot; afterwards rice boiled in milk was given.

As the object of my paper is simply to record the clinical history of an unusual and isolated outbreak of acute dysentery, I shall not load it with surmises as to whether *Amœba coli* of Lösch was present or not, as the infecting micro-organism,

and whether, therefore, the type of the disease justified the application of "amœbic dysentery," or true tropical dysentery (Osler, Councilman, and Lafleur). A *post-mortem* examination of the large bowel could alone have settled this question, and that was not possible under the circumstances.

Dr. Falkiner said he had a friend who suffered from chronic dysentery. He found the liquid extract of Indian bael of no use, but a preparation made by Squire, of London, from the fresh fruit was the only thing did him any good.

Dr. S. M. Thompson, from his experience in South America, said that the hippo treatment had failed. Enemata of starch and opium hastened death by blocking up the lower bowel. It was very contagious. Patients in the same ward got it, although fed on a different diet.

Dr. Dawson said that in acute dysentery an amœba had been discovered by Lösch. It had been found by other observers since. It grew very freely in an infusion of hay. It was not found in the Richmond Asylum cases referred to by Dr. Finny.

Dr. Little said there must be two or three different pathological changes which give rise to the symptoms of dysentery. He saw a great many cases in India. The astringent treatment was not useful in India but quite the reverse—such as calomel and castor oil. When he was there the ipecacuanha treatment was revived. In virulent acute dysentery it produced results unattainable by any other treatment. Vomiting following 20-grain doses was very rare, unless in the first dose or unless mixed with opium, as in pulv. ipecac. co. When pure and given in a little arrowroot it very seldom sickened. After the patient had an evacuation, 20 minims of liq. opii. sed., in about 2 drachms of warm milk, was injected with a small glass syringe into the rectum. If the patient had not a motion he was first given a warm water enema and subsequently the opium. It was generally followed by a few hours of quiet, which was a matter of considerable importance. In chronic dysentery he gave one-grain dose each of ipecacuanha and extract of hyoscyamus in pill—3 to 12 being given in the day, and put the patient on a milk diet. At about the same time as Dr. Finny's cases occurred there were several cases in the Adelaide

Hospital which presented all the symptoms of tropical dysentery, and which were confirmed by *post-mortem* examination.

Dr. Burgess said, when crossing the Atlantic, the temperature suddenly changed from being very cold to very hot. A great many of the passengers got symptoms of dysentery. Astringent and sedative remedies failed, and large doses of ipecacuanha caused vomiting. On the advice of one of the passengers, he tried a large teaspoonful of pulv. ipecac. co. They all rapidly got well under this treatment.

Dr. Finny, replying, said that when he stated that ipecacuanha had failed in the cases he mentioned, he did not wish to condemn it generally, as he had found it useful before. Osler mentioned several cases in which an amœba was found and called it amœbic dysentery, thus separating it from other forms of dysentery. He thought his own cases were probably more catarrhal than true tropical dysentery.

CASE OF PURULENT PERICARDITIS TREATED BY DRAINAGE.

By JOSEPH O'CARROLL, M.D.;
Physician, Richmond, Whitworth, and Hardwicke Hospitals.

[Read in the Section of Medicine, May 8, 1896.]

In eighty cases of paracentesis pericardii collected by Dr. Samuel West (*Med. Chir. Trans.*, Vol. LXVI., 1883), thirteen were instances of purulent pericarditis, and of these only two recovered—one a case of his own, the other recorded by Prof. Rosenstein. These two were the only ones which had been treated by free incision and drainage. I have not looked up medical literature for subsequent records, but incidentally I have come across a case of pyopericardium detailed by Dr. Hermann Bronner (*Brit. Med. Journal*, Feb. 14, 1891), which presents many resemblances to my own case in both history and treatment. I shall give the history of my case as briefly as is consistent with a fair presentation of it:—

Case.—Christopher S., aged twenty, a casual labourer, of intemperate habits, no fixed address and often without the means to procure a lodging—

1st day.—Was suddenly seized on the night of the 19th November, 1895, with great pain in his right side.

2nd day.—He was admitted to the Whitworth Hospital next day, which I shall call the second day of his illness. He was a thin, querulous, ill-nourished man. I found the right side of his chest as resonant as the left, but the expiratory sound seemed slightly prolonged and vesiculo-tubular in character, especially at the right interscapular area—no crepitus or creak. Diagnosis—right lobar pneumonia.

4th day.—A patch, giving tubular breath sounds in the infrascapular area. Otherwise no further localising signs.

6th day.—Had two attacks of epistaxis, with relief of a headache of which he had been complaining.

8th day.—Crepitus noted for the first time, at right interscapular area; suggestive of *crepidus redux*. Over the remainder of the lung, breath sounds seem more puerile and percussion sounds more tympanitic than on left. There is, in fact, no indication of a localised or delimited lobar dulness. At the infra-clavicular area, however, there is a patch of moderate dulness, and at the sternal end of the first intercostal space there is a fine crepitus with inspiration.

10th day.—He complained of pain in left infra-mammary area; pleuritic friction sound audible there. The area of præcordial dulness reaches 3rd rib. From this and a diminution in intensity of heart sounds and impulse, it was concluded that he had a moderate pericardial effusion. Morphin, $\frac{1}{6}$ grain hypodermically, relieved the pain, but it had to be renewed on each of five or six following evenings. Turpentine punch, and other stimulating expectorants were in use at this time.

14th day.—A second attack of epistaxis.

17th day.—Crepitations over the whole area corresponding to right lower lobe. Complaining of pain in his epigastrium.

During the next three days there was a great fall in temperature and pulse-rate without any diminution in number of respirations. Temperature fell from 103·4° and pulse from 156 to 97·5° and 60 respectively, while the respirations remained at 52.

20th day.—These latter figures were taken during my visit on the 20th day. It was manifest that we had to deal with some grave interference with, or inhibition of, the heart, toxic or mechanical or both. I was rather inclined to the theory of a toxic absorption from the lung, although attention was continually paid to the suspected pericardial effusion. During the next few days the pulse-rate increased to an average of about 120, while respirations fell below 50; the temperature stayed about 99°.

26th day.—On the 26th day, examination revealed that the right upper lobe was still dull; that the præcordial dulness extended to the upper border of left third rib, half an inch to the right of the sternum, in the 5th space, and half an inch outside the left nipple; and that the sounds were unduly faint.

29th day.—By the 29th day dulness had extended half an inch farther to right of sternum. The patient stated that pressure in the dull area, and especially in the infra-sternal notch caused him

pain, and he complained that he felt his heart jumping. His skin had a leaden hue of partial cyanosis. The pericardium was aspirated in the 4th left intercostal space, about midway between sternum and nipple, or perhaps rather outside this point, and pus to the amount of 30 oz. was withdrawn. This gave much relief; the epigastric tenderness was greatly diminished; the heart sounds became much more audible, and the dull area was reduced in its transverse measurement, though not in its vertical one.

On the 34th day, as I was satisfied that the dull area was again increasing, I had Sir Thornley Stoker to incise the pericardium for me. He did it at my aspiration puncture in the 4th space. There was a free outflow of pus, and no drainage tube was inserted till the next day but one (36th day).

At this date Dr. O'Sullivan, our pathologist, found Fränkel's diplococcus in the pus discharged from the wound.

From this onwards the patient had considerable relief, except for the dressing of the pericardial wound, which gave him always some pain. The introduction of the drainage tube had frequently to be preceded by dilatation of the sinus which gradually came to point from without backwards and inwards, as the anatomists would say, instead of directly backwards. In the process of dilatation I was able to satisfy myself that the pericardium on the heart was distinctly and painfully sensitive to the touch of the sinus-forceps.

On the 48th day the patient complained of pain in the left side, and we found evidence of some pleural effusion on that side. Fearing an empyema, due either to leakage at the drainage-wound or to renewed diplococcus infection, I inserted an exploration needle, and withdrew not pus but clear serum. This was to some extent satisfactory, but it was not very reassuring to find that the left lung, as well as the right, was acting as an impediment to a heart so gravely affected itself. Withdrawal of the fluid gave relief, but it was manifest that the heart was unequal to its work. While we had evidence that the pyopericardium had almost ceased to exist, so small was the daily quantity of pus discharged, it was noticeable that the general condition of the patient was deteriorating, and he finally died by simple failure of the heart on the 64th day of his illness.

The necropsy may be summarised by saying that, posteriorly and laterally, the pericardial layers were firmly adherent; in front was about a drachm of pus, and on the heart and on the parietal

pericardium a fibrino-purulent layer. The pericardium was greatly thickened, each layer averaging perhaps 3 mm. The right lung was tightly bound down by thick adherent pleuræ, and was in a condition of collapse in its lower portion, and of fibrotic induration in the upper; the middle lobe contained a plum-sized infarct. The left lung was adherent to the parietes on the outer and posterior aspect and for a small area anteriorly, while the intermediate pleural cavity contained about 6 oz. of clear serous fluid. The liver was nutmeggy; there were infarcts in both kidneys, and the peritoneum contained a few ounces of flaky yellow fluid.

This case is capable of discussion from so many aspects that I am almost deterred from going into any discussion of it at all. I shall content myself with saying that I consider it a case primarily of acute right pneumonia; that within a very few days the pneumococcus infection extended to the pericardium; that there were superadded to the previous symptoms those dependent not merely on weakened heart but those due to the mechanical bulk of the distended pericardium; that the left lung, having to do most of the work, was yet impeded in its function by passive congestion and a pleurisy, in part adhesive and in part serous; and that, finally, the "last straw" being heaped on by the locking up of the heart in a thick inelastic wrapper by cohesion of the pericardium, the circulation came to an end.

As to the treatment, I am satisfied that the pericardium is as tolerant of free drainage as the pleura, perhaps more so. The temperature, pulse, and respiration records of the case show absolutely no disturbance which could be referred to the drainage. Regular washing out of the pericardial cavity was not resorted to; it did not seem to be required, and as it turned out, obliteration of the cavity was almost complete when death occurred; but I may say that I should have no hesitation in washing out the pericardium if I thought it useful, provided that it was done siphon-wise at minimum pressure and with a perfectly free outlet. In fact, on one

occasion in this case I did flush out the pericardium in this manner, using a double drainage-tube, and not an antiseptic but an alkaline fluid (bicarbonate of sodium), in order to thin the pus and make it flow out more easily.

With regard to the site of the incision, I am of opinion that it would be better in future cases to enter the pericardium closer to the edge of the sternum—say about three-quarters of an inch from it. In that position the knife avoids the internal mammary artery, on the one hand, and the pleura, on the other, while the subsequent drainage sinus is not likely to be as long as when the outer opening is further away from the normal position of the pericardium. In Dr. Samuel West's case the incision was also made rather far out, and a long sinus formed. But in neither his case nor mine was there any leakage into the left pleura; in both cases an adhesive pleurisy in the infra-mammary area had occurred before the initial paracentesis, and this effectually shut off the pleural cavity from the drainage sinus. Dieulafoy's advice, based on observations on the pericardium distended experimentally *post-mortem*, is to puncture in the 5th space about an inch from the left edge of the sternum. In my case both the initial paracentesis and the subsequent incision were made in the 4th space, and this space was also chosen for the drainage incision in the three cases reported respectively by Rosenstein, West, and Bronner.

With regard to the symptomatology of purulent effusion in the pericardium, it may be useful to say a word or two. In my case fever was present, and moderately high during the first eighteen days; then both temperature and pulse-rate fell to normal or thereabouts for ten days, during which time the respirations became more than twice, and for some days three times as frequent as they should be normally. That is to say, that increase of the cardiac area of dulness, with serious quickening of breathing, point to pericardial

effusion, and this may be purulent, even though there be no pyrexia.

A large pericardial effusion secondary to pneumonia may be suspected to be purulent, since we know of that sequence in other parts, but we have additional hint of suppuration in præcordial, and more especially perhaps in epigastric, tenderness. The lesson which I particularly desire to draw from the case is that free drainage of a pericardium full of pus is as legitimate and safe a procedure as drainage of an empyema. I have not the smallest doubt that my case would have recovered but for his previous ill-nutrition and the initial disablement of his right lung.

Dr. W. G. Smith said he thought Dr. O'Carroll's paper gave great encouragement to the early exploration of similar cases, as the operation seemed devoid of danger, and when performed in time gave satisfactory results.

Dr. M. A. Boyd related the particulars of a case in which he drew off four ounces of serous fluid and the patient recovered. The temperature might be normal, with pus in the pericardium. In children broncho-pneumonia often preceded the collection of fluid in the pericardium. He thought that paracentesis should be resorted to more frequently than at present in cases of pericarditis in children.

Dr. Doyle asked was the pericardium sutured to the integument in the surgical operation succeeding the operation of tapping, as if so this might account for the secondary infection of the left lung.

Dr. Heard drew attention to the remarks of Dr. Ewart, of London, that a patch of dulness might be detected at the inner side of the angle of the left scapula at the base of the left pleura as an early symptom in these cases.

Dr. Drury asked Dr. O'Carroll to mention the exact site selected for the operation, as it had been recommended some time ago to tap the pericardium as close as possible to the left margin of sternum in the 4th interspace. He had examined a large number of subjects in the dissecting-room, T.C.D., and found that in a considerable proportion of cases either the internal mammary artery or vein would be wounded by such a proceeding.

Dr. Finny said that in his long experience he had only met with one such case of suppurative pericarditis requiring operation. The result was fatal, the fault being one of omission rather than commission; as, though he tapped the pericardium 1 inch outside the sternum, between the 4th and 5th ribs, and drew off 8 ounces of purulent fluid, giving immediate relief, a second attempt to perform the same operation with a larger needle did not succeed, as for some unaccountable reason he failed to reach the pericardium. The *post-mortem* examination revealed a large collection of pus in the pericardium. He thought that pericarditis was comparatively a rare disease, and extensive pericardial effusion was very rare, and consequently the cases requiring tapping were very few; but when the symptoms did not yield to other forms of treatment tapping should be resorted to, and especially so when the contents were believed to be purulent in nature.

Dr. O'Carroll, in reply, said that he left the surgical details to Sir Thornley Stoker, who performed the operation for him. There was no infection of the left pleura, as the fluid which it contained was a clear serous fluid due to the cardiac failure which occurred at the end of the patient's life. Though he had carefully examined the patient's chest he had not remarked the symptom referred to by Dr. Heard. In reply to Dr. Drury, he stated that in making his puncture he had selected a point in the fourth space a thumb's breadth internal to the nipple line.

LEUCOCYTHÆMIA.

By JAMES CRAIG, M.D., F.R.C.P.;
Physician to the Meath Hospital.

[Read in the Medical Section, May 8, 1896.]

A BOY died in the Meath Hospital on the 27th of January last, a victim to the comparatively rare but generally rapidly fatal form of leucocythæmia, known as the lymphatic type.

The purely splenic, or lymphatic, or medullary form of leukæmia is seldom met with; more usually mixed types occur, in which two or perhaps all three varieties are combined, the commonest of all being the spleno-medullary, but still when you have examined the films of stained blood that are placed under the microscope, I think you will agree with me in regarding this case as belonging to the purely lymphatic type. In a paper read before the Medical Section of this Academy in 1891, Dr. Wallace Beatty recorded a case of the purely medullary form, and for the recognition of which, in the absence of external signs to prompt him, he deserves the greatest credit. Unfortunately, at that time but little had been done in the direction of distinguishing the different kinds of leucocytes to be found in the blood, so that although these cells were, as a whole, greatly increased, still the particular variety met with in that case has not been recorded.

The boy, who was nineteen years of age, was an Italian by birth, an image-maker by occupation, and his fatal illness, which was of an acute nature, extended only over two months altogether.

On observing the photograph [exhibited] you will note particularly the thickness of his neck, the swelling of the parotid region, the tumours in the axillæ—all of lymphatic origin—and the distension of his abdomen. The enlargement of lymphatic

glands was universal. The spleen and liver were both increased in size, and the tonsils were swollen and painful.

The pallor of his skin was probably accentuated by a naturally sallow complexion peculiar to his countrymen. As the illness progressed, dyspnœa became more marked. Profuse epistaxis occurred, and with this a diarrhœa so persistent that the treatment of the case was mainly directed to combating these two distressing symptoms. Severe epigastric pain was constant, which, with evident intestinal distension, as well as the persistent diarrhœa, suggested an inflammation of the colon that was not verified by the *post-mortem* examination. There was no tenderness over the sternum.

Priapism—an occasional symptom in such cases—was observed. A venous hum was present in the neck, and a soft systolic murmur was audible over the mitral area. About a week before his death the lower extremities became œdematous, and the glandular swelling was considerably diminished. The temperature was variable, ranging from 99° to 103° F., and as death approached it fell to 97° F.

The case on admission was thought to be one of Hodgkin's disease, or lymphadenoma, and undoubtedly many of the cases described as such were similar in nature to the present one; but if we take it that permanent increase of white blood cells is characteristic of leucocythæmia, while mere diminution of the cellular elements of the blood is a feature of the other, then we are in a position to discriminate between these closely-allied affections—lymphatic leucocythæmia, and lymphadenoma.

I will not weary you with details of the repeated examinations of the blood which were made; suffice to say that the red cells were reduced to at least one-third of their normal number—a condition for which the epistaxis was probably largely accountable, while the white ones were both absolutely and relatively increased, the proportion of white to red being about one to twenty instead of one to three hundred.

In the slides which are placed under the microscopes, and for the staining of which I am indebted to Dr. M'Weeney, you will observe at a glance this increase of white cells, but in addition to the increase it is possible also to distinguish between the different varieties of these corpuscles. One preparation is stained with acid hæmatoxylin and eosin, and the other with Ehrlich-Biondi triple stain of methyl green, acid fuschin, and orange.

You are aware that the leucocytes of human blood are not all of the same size, and that they differ also in other respects. In some the protoplasm is granular, in others it is without granules; in some the granules are large, in others, again, they are small. But, in addition to these peculiarities, it has been found that the granules in some cells stain with acid dyes, while in other cells they stain only with basic dyes. I may add that the latest investigations in this connection go to show that the granules of certain cells, which were supposed by Ehrlich to stain with neutral dyes alone, and hence called by him neutrophiles, are now found to stain not with neutral but with acid dyes.

The most recent work on the subject is a paper by Kanthack and Hardy. They classify these cells into five varieties, as follow:—

Acidophiles, or Oxyphiles, granules stain with Eosin	1. Coarsely granular, polynuclear or horseshoe-shaped nucleus (Ehrlich's eosinophiles)—2·3 per cent.	10·0 $\mu\mu$ in dia.
	2. Finely granular, irregular-branching nucleus (Ehrlich's neutrophiles)—62 per cent.	8–9·0 $\mu\mu$,,
Basophile, granules stain with Methylene blue	3. Finely granular, trilobed nucleus—0·7 per cent.	7·0 μ ,,
	4. Hyaline cells, *free from granules*, mononuclear, and generally spherical—11 per cent.	8·5–10·0 $\mu\mu$,,
	5. Lymphocytes with single nucleus, a very little protoplasm—24 per cent.	6·0 $\mu\mu$,,

The percentages here represent the proportion of each variety found in the blood in health.

All of these cells occur in normal blood, and, roughly speaking, in splenic leukæmia No. 1 are greatly increased, in the lymphatic form the increase is in No. 5, while in the medullary form other cells are observed that are not present in health, namely, mononuclear cells, larger than No. 4, but unlike them in containing granules which stain only with neutral dyes. These are called myelocytes, from their supposed origin in the bone marrow.

Now, in these films, prepared from the Italian boy's blood, you will notice that, practically speaking, the small lymphocytes only are increased, although a few of at least 1, 2 and 4 can be seen as well, but the total increase of leucocytes in the lymphatic form is never found to be so great as in the splenomedullary form. So much for the blood.

The main facts revealed by the autopsy were these :—The organs generally were pale and bloodless, all the subcutaneous lymphatic glands were enlarged, some not larger than a marble, others the size of a walnut. About a quart of serous transudation lay in the pleural sacs and a few ounces in the pericardium. A mass of lymphoid tissue lay in front of the lower end of the trachea, and a larger mass surrounded the right bronchus. The lymphatic glands along the whole course of the intestinal tube were much enlarged, the liver and spleen were considerably increased in size, the pancreas was white and bloodless, the stomach and intestines were distended with gas, and the mucous membrane of the latter was pale, while Peyer's patches and the solitary follicles were enlarged and sharply defined. Under the microscope the liver and spleen were found to be impacted with lymphocytes, which accounted for their increased bulk.

The case is of interest in several particulars. In the first place this lymphatic variety is rare, and where it does occur

is generally, as in the present instance, found in young subjects, and it usually runs through a rapid and fatal course.

In the splenic or splenomedullary forms the insidious and slowly progressive nature of the affection, as it is commonly met with, affords an opportunity for treatment, so that cases of improvement or recovery are not infrequent. I have several such patients whose blood has improved and whose spleens have returned almost, if not quite, to their normal dimensions under the free administration of arsenic, so that they are able, after a number of years, still to follow their daily avocations. In this case, however, I was obliged to drop the arsenic on account of the diarrhœa, and to treat symptoms instead.

In the next place, the acute nature of the symptoms pointed to an infective cause, and it is highly probable that before long we may have the presence of a micro-organism, or its poisonous products, demonstrated in the blood to account for the grave changes that are present in leucocythæmia.

And lastly, the advances that have been made within recent years in the study of the white-cell elements of the blood render it a fairly easy matter now to distinguish in stained preparations the particular form of the affection under observation, although it is true that much has yet to be added before our knowledge becomes at all perfect.

Dr. W. G. Smith asked if arsenic had been administered in this case, or if the treatment by marrow fat had been tried. He laid stress on the great advance that had been made in the microscopic examination of the blood in these cases, and thought that it should more generally be carried out in hospital practice, as the process was not a difficult one; he thought that the specific significance clinically of the various leucocytes was still very doubtful. He stated that the continuous use of arsenic in chronic

cases was not devoid of danger, as peripheral neuritis might follow as a result. He mentioned the great chemical changes that occurred in the urine in this disease. The urić acid increased and xanthine bodies also increased. The normal oxidizing power of the body was not seriously affected.

Dr. Boyd mentioned that he had treated a case of the lymphatic form, in which there was marked tenderness of tibiæ, sternum, and ribs. Under arsenic there was distinct improvement (proportion of leucocytes 1–300). The patient was then put on bone-marrow, but distinct relapse occurred (leucocytes 1–20). On returning to the arsenic treatment improvement again took place, and the patient returned to the country, where, however, he relapsed again, and died in three or four months.

Dr. Craig, in reply, said that he had tried arsenic, but as it caused diarrhœa and pain in the stomach, he had to cease administering it. He had thought of using marrow fat, but had to content himself with trying to keep his patient alive.

NOTES ON CLINICAL CASES.

By ALFRED R. PARSONS, M.B. Univ. Dubl., M.R.C.P.I.;
Diplomate in State Medicine;
Physician to the City of Dublin Hospital.

[Read in the Section of Medicine, May 8, 1896.]

I. CASE OF EXTENSIVE EMPYEMA.

Case.—P. D., aged sixteen, a telegraph messenger, was admitted to the City of Dublin Hospital on 2nd January, 1896. He was carried in on a stretcher about twelve noon. On examination his face was flushed and slightly cyanosed, his respirations were very frequent, and the pulse, which was very small in volume, was about 150 per minute. Inspection of the trunk showed that the right half of the thorax was fuller and rounder than the left, and almost motionless, while the left half was moving actively in respiration. There was well-marked cardiac pulsation as far out as the left mid-axillary line. On palpation the edge of the liver was felt as low down as the umbilicus. Percussion gave, on the right side, an absolutely dull note from the clavicle to the lower margin of the displaced line. This area of dulness extended from one-half to one inch beyond the left margin of the sternum, and from the level of the third rib passed still further outwards, owing to the sinistrocardia. The aortic second sound was not heard till one had crossed the left margin of the sternum; and although the action of the heart was tumultuous, no abnormal sound could be heard over any of its orifices. There were no respiratory sounds audible over the dull area, but the breath sounds were puerile over the left lung.

History of the case.—He states that he first felt ill five weeks before his admission. His illness commenced with great headache and giddiness. Three days later he had severe pain in the left side of his chest, which changed on the following day to the right side. He was confined to bed from the commencement of his illness. Mr. Fitzgibbon, who saw him a day or two after the onset of the attack, considered that he was suffering from influenza, which, a few days later, was complicated by an attack of croupous pneumonia of the lower lobe of the right lung. When P. D. was apparently nearly convalescent from pneumonia, Mr. Fitzgibbon,

owing to becoming ill himself, was obliged to hand over the case to Dr. Boyce, who kindly sent him into hospital under my care.

There is no history of any previous illness, and his family history is good.

The severe dyspnœa, the great displacement of the heart and liver, and a pulse of very small volume and almost uncountable, rendered immediate interference necessary. The physical signs indicated the presence of a large effusion into the right pleural cavity. This diagnosis was confirmed by an exploration with a small syringe. The fluid obtained was pus. His condition was such that it was not thought advisable to give him either chloroform or ether for even a few minutes, or to submit him to the pain attendant on the introduction of a drainage tube into one of the intercostal spaces without anæsthesia. It seemed that the least painful procedure would be to introduce a medium-sized trochar and cannula. The site selected was the sixth intercostal space, a little posterior to the mid-axillary line. The pain was diminished by freezing the skin. One end of a piece of rubber tubing was fastened to the cannula on withdrawal of the trochar, while the other extremity dipped into a vessel containing a measured quantity of boracic lotion. The pus flowed away in a steady stream; and, as no air entered the pleural cavity, the expansion of the lung and the return of the displaced viscera were facilitated. From 2 30 p.m. on the day of his admission till 9 30 the following morning over six pints of pus were collected. It was thick, creamy, and without any fœtor. On microscopical examination some capsuled diplococci were recognised, but no staphylococci or streptococci. On this date the urine was of a light straw colour, slightly alkaline, and contained albumen and a sediment of pus corpuscles. The evacuation of the fluid gave almost immediate relief to the patient; his respirations falling to 20 and his pulse to 84. The next day it was noticed that his liver had resumed almost its normal position, while the apex beat of the heart was to be found a little internal to the mid-axillary line. During the succeeding twenty-four hours only about five ounces of pus escaped, probably owing to some obstruction of the cannula by a coagulum, but on the second day after his admission the flow was re-established, and another pint of pus escaped. The right side of the thorax had now lost its distended appearance, the intercostal spaces were again perceptible, cardiac pulsation was less marked than previously, and the apex beat was an inch nearer its normal position.

On 7th January there was a slight return of the respiratory sounds in the first right intercostal space, while below the second rib the breath sounds and vocal resonance were almost entirely absent. The pus continued to escape in small quantities, and on the following days the breath sounds were audible in the second right intercostal space. Pus and albumen had almost entirely disappeared from the urine by this date. As the discharge from the cannula was now very slight, and the pus was escaping at its side, it was felt that the time had come for more radical measures than those of which the boy's condition had permitted at the time of his admission. Consequently, on 10th January a large rubber drainage tube was inserted in the seventh intercostal space a little posterior to the position of the cannula. The patient was frequently dressed by my resident pupils, Messrs. Taylor and Cunningham, to whose great care the subsequent satisfactory course of the case is largely due. The tube was finally removed on 16th February. The heart slowly regained its normal position. On 21st February there was good resonance in the first three intercostal spaces, somewhat impaired resonance in the fourth, and a dull note in the fifth space respectively. From this date a marked improvement took place in his general condition, and from being feeble and emaciated he became robust and fat. Early in April he was sent to the Convalescent Home, and some weeks later he was able to resume his work at the Post Office. He was exhibited before the Medical Section of the Royal Academy of Medicine on the 8th May, and then his right side was only very slightly flatter than his left, and expanded remarkably well on inspiration.

The following points are, I think, of some interest in connection with the case :—

1. *The extent of the empyema.*—The right thoracic cavity contained probably, at the lowest calculation, ten pints of pus. Notwithstanding the displacement of the diaphragm downwards, and of the mediastinum and heart to the left, there must have been great compression of the right lung to make room for such a quantity of fluid.

2. *The œtiology of empyemata.*—In the case under consideration the formation of pus was subsequent to an attack of croupous pneumonia—not an uncommon antecedent of an

empyema. In the pus was to be found the diplococcus of pneumonia, which, as Fränkel and Weichselbaum have pointed out, is present in about ninety per cent. of the cases of croupous pneumonia. Other bacteria, especially staphylococci and streptococci, and tubercle bacilli have been detected in the pus of empyemata, but cases excited by these organisms are not so likely to run a satisfactory course as those produced by the Diplococcus pneumoniæ.

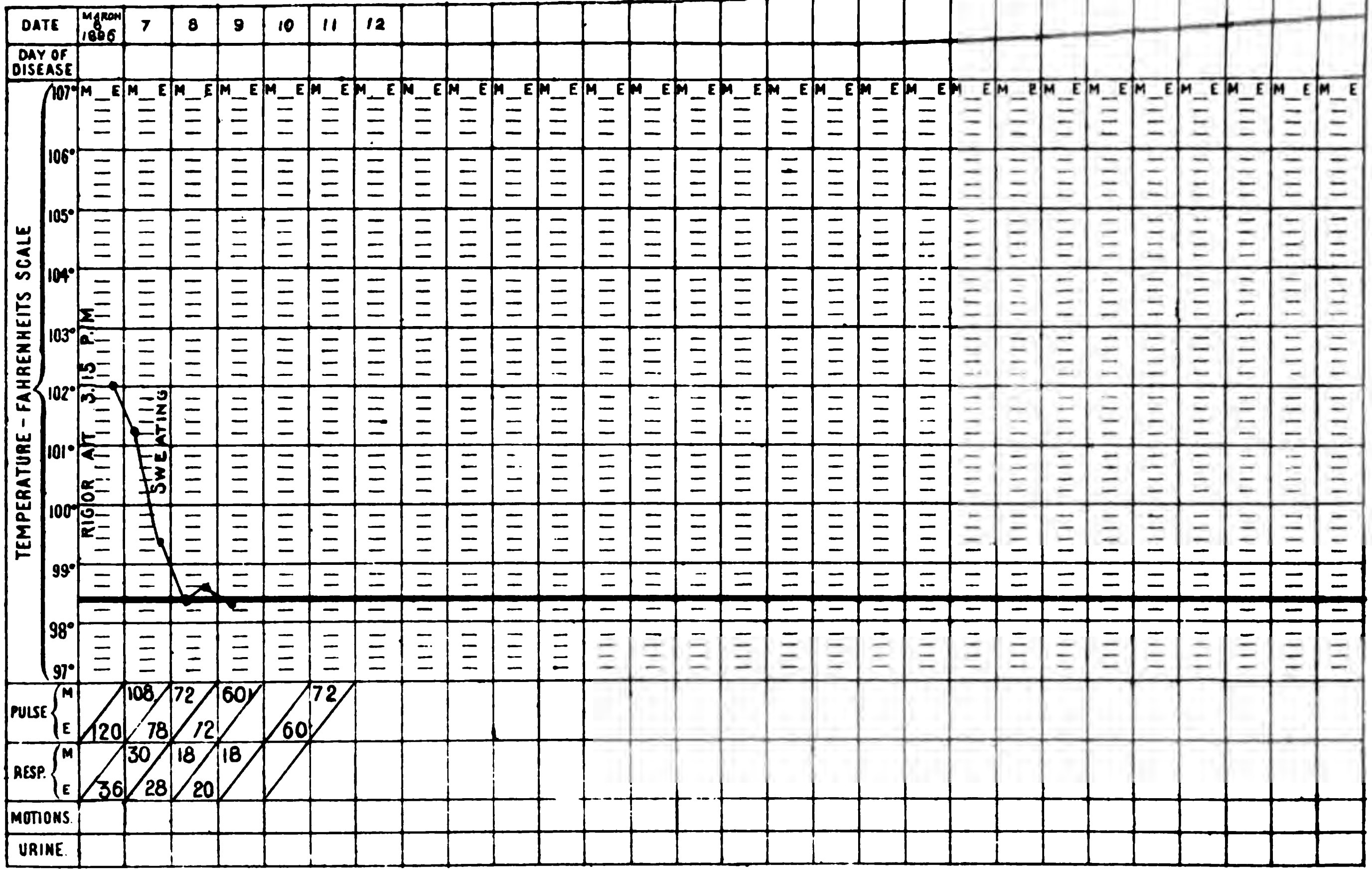
3. *Pyuria associated with empyema.*—This is a condition to which I have not been able to find any reference. I have noticed it in at least one other case of empyema following on pneumonia. In the case under consideration at present both pus and albumen disappeared shortly after the evacuation of the fluid from the thoracic cavity.

4. *The treatment.*—It is only necessary to point out that, notwithstanding the great extent of the pyothorax, it was not necessary to resect a rib, and still less to remove portions of several ribs. The wound was frequently and carefully dressed with boracic powder and antiseptic wool. The discharge always remained sweet, and no attempt was ever made at washing out the pleural cavity.

II. CASE OF CROUPOUS PNEUMONIA.

Case.—A. B., aged sixteen, a schoolboy, was in excellent health till the afternoon of the 6th March, 1896, when, at 3 15 p.m., he got a shivering. He did not complain of this, and walked with the other pupils to the field for athletic exercise, about one mile distant from the school. At 4 p.m. he vomited, and returned to the school. At 5 p.m. he was sent to bed, as he was complaining of pain in the lower part of the left side of his thorax, and also of some cough. At 9 30 p.m. his temperature was 102°, pulse 120 per minute, and respirations were 36. There was no herpetic eruption about the mouth. The tongue was large and slightly coated, and the glands along the posterior border of the right sterno-mastoid muscle were very slightly enlarged. On inspection of his thorax nothing abnormal was noticed. Percussion did not disclose any difference between

Name. A.B. Age 16. Disease, Croupous Pneumonia. Result. Recovery.



the two sides, except that the note over the lower part of the left lung behind seemed a little tympanitic. On auscultation nothing abnormal was noticed except a suspicion of a little friction on deep inspiration over the base of the left lung. The cardiac sounds seemed quite normal. Shortly after the examination of his chest was concluded he had a fit of coughing, which resulted in the expectoration of several masses of mucus stained a bright blood-colour in the centre.

He was given milk and soda water, and a mild diaphoretic mixture, and a poultice was applied to his side.

7th March.—Had a very good night and feels better. The pain in his left side is less. There is still no herpetic eruption. On examination the note in the left infra-clavicular region, and also over the base of the left lung behind, was a little more resonant than that obtained in corresponding places on the right side. There was, however, no marked auscultatory difference, nor was any alteration in the vocal fremitus detectable. Temperature, 101·2°; pulse, 108; respirations, 30. He coughed up one rusty-coloured, very tenacious sputum, which was removed, stained, and, on microscopical examination, found to contain several diplococci enclosed in capsules.

At 8 p.m. he was sleeping, and his respirations were 28 per minute. He awoke in a few minutes, and was found to be perspiring. He said that he felt much better, and that the pain was nearly gone. Temperature, 99·4°; pulse, 78. The physical signs on percussion were similar to those of the morning; but on auscultation there was slight intensification and prolongation of the expiratory murmur, with increased vocal resonance over the lower part of the left lung posteriorly. Two more sputa, similar to that of the morning, were expectorated during the day. Still no herpetic eruption.

8th March.—Temperature, 98·4°; pulse, 72; respirations, 18. Very slight herpetic eruption about middle of lower lip. The vesicles are not quite so large as the head of a pin. He slept very well during last night, and has, practically, no pain. The physical signs in his chest are, if anything, less marked than yesterday. At 7 p.m.—Temperature, 98·5°; pulse, 72; respirations, 20. One rusty sputum during the day.

9th March.—Slept all the night; no pain on deep inspiration; herpetic eruption much more marked. Temperature, 98·4°; pulse, 60; respirations, 18. No physical signs in lungs.

10th March.—Up for an hour; feels very well. I could detect no physical signs in his lungs. He expectorates a little white, frothy mucus.

11th March.—Feels perfectly well; is up, and wants to resume his ordinary school work.

The following points will be noted from the clinical records of this case:—

(*a*) Its very short duration. About thirty hours elapsed from the rigor to the completion of the crisis which took place by sweating.

(*β*) The herpetic eruption did not make its appearance till the fourth day of the illness, when the temperature had been normal for over twenty-four hours.

(*γ*) The total number of rusty sputa was about twelve. They were exceedingly viscid.

(*δ*) The absence of dulness, and the presence of a slightly tympanitic note over the upper lobe of the left lung in front, and over the lower lobe of the same lung behind. This note, which, for want of a better name, I have called "tympanitic," is a most valuable sign in pneumonia. Its explanation is difficult, and it has been suggested that its peculiar character is due to the relaxation of the neighbouring pulmonary tissue, especially the alveolar walls, owing to the pressure exerted by the increased size of the portion of the lung which has become consolidated.

Dr. W. G. Smith said that in such cases the heart was generally the first organ to return to its normal position. After aspiration, the effects of the diplococcus of pneumonia seemed more amenable to treatment than those of the streptococcus. The examination of the pus drawn off was therefore of the greatest consequence. He agreed with Dr. Parsons that a peculiar tympanitic note on percussion with roughness on breathing, were often the first symptoms in such cases as he related.

Dr. Doyle questioned whether the heart was not now displaced to the right, and the right lung still considerably unexpanded. He agreed that good results followed washing out the pleural cavity with antiseptic solution.

Dr. Parsons, in reply, said that he did not claim that the organs had fully recovered both position and function, but considered that the improvement, so far as it went, was remarkable.

SECTION OF SURGERY.

ON THE MANAGEMENT OF CASES DURING THE PERIOD IMMEDIATELY FOLLOWING OPERATION.

By SIR THORNLEY STOKER, M.Ch., R.U.I., *Honoris Causa*;
President, Royal College of Surgeons, Ireland.

[Read in the Surgical Section, November 8, 1895].

When I last had the honour of addressing you from this chair, I took occasion to apologise for speaking on a vulgar instead of an obscure or novel subject, and I am again disposed to use my opportunity to a like effect. I do not presume to address myself particularly to the more senior portion of my audience, but there are a large number of the younger members of the Academy present, and to them I venture to say something about a subject which exercised me much in my younger days—which does so still—and which is important because of its very commonness, and worthy of attention because it is sometimes overlooked.

I refer to the management of patients during that often-time critical period between the performance of a surgical operation and the moment at which, the risks consequent on it having passed, the patient becomes free from immediate and non-particular danger. This is the period occupied by two conditions of depression common to any or all operations, and therefore to be considered by themselves, apart from special dangers proper to individual surgical procedures. They are—

a. Shock.

b. Exhaustion, consequent on vomiting.

They may not be present, or being present, may vary in duration from a few minutes to several days. Their intensity cannot be foretold; it may be but slight, or may be grave even unto death, and nothing in the condition of the patient, the nature of disease, or the character of operation can afford any reliable evidence as to their possible severity. They may be serious in those who are vigorous, and unimportant in those who are weak; the very young and the aged may suffer from them but slightly, and persons of middle life may be in peril; they may follow the most trivial operation, and be absent from the most severe.

Those who have had experience in surgery know how often they meet surprises in the immediate consequences of operation. Sometimes the experience is the pleasant one of finding the treatment borne better than was expected; sometimes it is the sad one of danger following an operation when it was not looked for. The lesson we learn is, never to neglect during and after operations every detail which can lessen danger by diminishing the chances of shock or exhaustion, and which can assist subsequent reaction.

The causes which tend to produce shock are numerous, and, for the most part, obscure. There are many states of enfeebled health, of particular debility, and of other recognised conditions which predispose to it. But there are unseen, and often unexpected circumstances, which may lend themselves to its production. Operative measures should, therefore, never be approached without the most careful general examination of the patient, and the most thoughtful consideration of his physical position. It is impossible to formulate exact laws in this direction; these perceptions are largely matters of experience, and only time and clinical study can develop them. But from long observation we know that such considerations of the general state of a patient about to be submitted to operation are often overlooked or

disregarded, and I suppose most of us have had bitter regret of our want of perception.

In addition to general matters concerning the patient, there is one particular which is proper to the surgeon, and in which great error is common, I refer to the undue prolongation of operations. Two circumstances have, in our generation, lent themselves to this; one is the facility afforded by the use of anæsthetics; the other the extreme care, conservatism, and attention to detail, begotten by the introduction of antiseptic methods. There is no one circumstance which so tends to the production of shock and to the exhaustion produced by subsequent vomiting as prolonged anæsthesia. I have had this impressed on me by severe lessons, and I am satisfied that we should watch ourselves very closely in this direction, and sacrifice detail and conservative measures in instances where they demand the unsafe prolongation of anæsthesia. I can look back on cases where I regret the time spent on an operation; for instance, the only example of elected operation for the radical cure of hernia which has ever proved fatal in my practice, was one in which, in my anxiety to conserve the testis in an extremely complicated rupture, I prolonged the dissection to such an extent as to produce shock from which the boy never rallied. I know now that had I removed the organ I could rapidly have concluded the treatment and saved the patient.

What I have said of the production of dangerous shock is largely true of exhaustion immediately following operation. Given the conditions, known and unknown, which predispose some patients to it more than others, prolonged anæsthesia with its often attendant vomiting, is its most usual provocative. We generally see post-operation vomiting proportionate in severity and persistence to the duration of anæsthesia.

So much for causes of shock and vomiting; what of the

means of avoiding them, and of relieving them if they occur? It is a truism to say that prevention is better than cure, and it will be seen from what I have said that a due despatch in the performance of operations, and a consequent shortening of the period of anæsthesia, is the most important direct measure to attend to. To an audience like this I need not elaborate this part of my subject; indeed, I could not if I would, for there are a hundred various conditions and circumstances which must guide us, and their knowledge and application are among the matters which make the difference between one surgeon and another, and which lend themselves to our successes or contribute to our failures.

Shock and vomiting are so much due to the same causes, and so subject to the same remedial measures, that it is difficult to consider them apart. I have learned to rely on three agents only for the relief of shock—

a. Heat,

b. Alcohol; and

c. Morphia or opium.

Concerning the first of these, we generally find that if the surface of the body can, by the use of hot applications, be brought to a wholesome warmth, the danger has been overcome. If to the use of hot water, contained in bottles, or, better still, in small Indiarubber bags, we add rubefacient applications of mustard to the extremities, about the region of the solar plexus, or over the heart, we have a valuable armament for the inducement, not only of heat, but also for the production of a physiological stimulation.

If these measures fail, or response to them is too slow, alcohol must be resorted to. It should be given by the rectum, not only because vomiting may exist, or may be induced by the introduction of stimulants into the stomach, but also because when shock is present the stomach has little or no power of assimilation. The possible necessity

for rectal stimulation and alimentation after operation is one of the reasons why the bowel should always be thoroughly cleared before any procedure, however slight, in which anæsthesia is to be employed.

Failing by these measures to induce reaction, opium or morphia has to be resorted to. If rapid effect is sought morphia is the most useful agent, and it is also that which best controls vomiting. But where there is no vomiting, and where the stimulating effect of the drug is our chief aim, opium itself is to be preferred; it must be given by the rectum. No rules can be laid down for the dosage in these cases, beyond the two points that are to be borne in mind—first, that opium is tolerated in large quantities by persons suffering from shock, very much as those bear it who have lost much blood; and second, that it must be given intelligently, that is to say, given watchfully, dose following dose, until the due effect is produced. The value of morphia given hypodermically, or of opium administered by enema in all cases of depression due to shock, cannot be overstated. I can look back on lives lost after operation from shock and exhaustion, which greater clinical experience and more courageous use of morphia or opium would now enable me to save. If it be sought to study the effect of opium as a stimulant, it cannot be better seen than by watching its effects in the shock and collapse following extensive burns. I have to thank the help afforded by modern nursing development for much wholesome change of opinion in these matters. It has only been since the introduction of trained women to the bedside, with their watchful intelligence and careful skill, that I have learned properly to deal with cases after operation. That intuitive faculty, amounting to what used to be miscalled an "instinct," and which is the special property of women, finds its best expression in the study and management of difficulties like those I speak of.

Of the treatment of the usually more persistent condition of vomiting, with its attendant exhaustion, much need not be added to the limited list of measures employed in the treatment of shock. Warmth, rectal stimulation, and the hypodermic syringe again find their use. If to these I add rubefaction, or occasionally limited blistering over the stomach, I have enumerated all the means I find most excellent. The host of drugs recommended to check vomiting, are, in my experience, worse than useless; they are not only ineffectual, but they add seriously to the gastric disturbance. One measure, introduced of late, we are trying in the Richmond Hospital. I mean the use, for some hours after operation, of a mask charged with vinegar. We are not yet in a position to speak definitely about this treatment, but it seems to give promise of success in cases where chloroform has been the anæsthetic. The rationale of its action is simple; the chloroform which is exhaled by the lungs is decomposed during expiration into chlorine and formic acid. The chlorine, by its irritation of the trachea and larynx, is probably the cause of vomiting, and being taken up by the acetic acid, is rendered harmless. But so far as present experience carries us, the hypodermic use of morphia is the cardinal remedy for the species of vomiting of which I speak. It must be used intelligently, fully, and fearlessly, and will exercise its effects both as a stimulant and gastric sedative far beyond those of any other remedy.

If it be conceded that it is useless to introduce stimulant or aliment into the stomach of a patient suffering from shock, it will be granted that it is not only useless but improper when vomiting is present; and here a previously-cleaned rectum serves its owner well. Stimulant associated with such nutriments as can be readily absorbed, should be periodically introduced. It is generally found that vomiting any more than shock does not persist with a warm surface, and therefore the three remedies I have indicated should be

employed in the same sequence as in shock; first, heat; second, rectal stimulation and nourishment; and third, morphia.

The distressing symptom of thirst is one which gives great trouble. There is nothing I have learned to discredit more thoroughly for the relief of thirst than ice. It increases thirst both directly, and by the induction of vomiting. The emesis it produces is due to the quantity of water the sucking of ice insidiously introduces into the stomach. Nothing should be put into the stomach in these cases; so that ice is not only useless, but hurtful. Thirst is best relieved by frequent rinsing of the mouth with hot water, and by the occasional introduction of two or three ounces of warm water into the rectum.

In what I have said I have avoided speaking of details and methods; I have remembered that although some of us are younger and less experienced than others, all of us in this room are educated in the factors of surgical science, and I have therefore tried to be suggestive rather than didactic, and to avoid details except in so far as they were necessary to the elucidation of principles.

If I may reduce to formulæ the matters I have referred to, I would put them thus:—

1. That the tendency to prolong operations must be carefully guarded against, as it is a grave cause of danger.

2. That in the treatment of shock and vomiting following operation, we get no help from the stomach, and must rely on the rectum as its substitute.

3. That heat, alcohol, and opiates are our best remedies; and that the latter are well borne, and must be intelligently used to their full effect.

4. That drugs of the class ordinarily used to check vomiting are of little or no use in the cases under consideration.

5. That ice does not relieve thirst, and does harm by introducing water into the stomach and so provoking vomiting.

COLOTOMY AND CONDITIONS REQUIRING SUCH OPERATION.

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In a communication such as this it may not be uninteresting to briefly allude to the history of the operation of colotomy prior to recording personal experiences and results therefrom.

It cannot be doubted that operative measures for the treatment of intestinal obstruction were practised by the Greeks, for Bonet, who revised Barbette's work, writes that operations for volvulus were successfully performed; and in the Transactions of the Royal Academy of Paris, Vol. IV., M. Hevin alludes to Bonet's observations; and in the History of Medicine, by Le Clere, Praxagoras is accredited with recommending laparotomy for the cure of "volvulus of intussusception," he not making a distinction, and in his time also the intestine was opened for the escape of fæces, presumably a form of colotomy.

Littre, in 1710 (Mem. Acad. de Paris, Vol. X.), was the first to suggest colotomy by opening the sigmoid flexure in the left inguinal region through the abdomen; it was proposed for cases of atresia ani. Whether this operation was ever performed by Littre is not known, but in 1776 Pillore, of Rouen, opened the cæcum in the right inguinal region; and about the year 1800 Callisen advised the opening of the colon by an extra-peritoneal operation in the left lumbar region. He failed to open the gut upon the dead, and he is not recorded to have practised this

operation upon the living. About this period Fine, of Geneva, appears to have performed transverse colotomy, and Amussat, after him, performed six retro-peritoneal operations upon the right side with five successes; while Ashmead, of Philadelphia, in 1842, performed on the left side lumbar colotomy (retro-peritoneal), evidently not being then aware of Callisen's proposed operation.—*Trans. Coll. Phys.*, Philadelphia, 1842.

In England, Curling, Bryant, and Allingham were among the earliest advocates of colotomy; in Dublin, Dr. Kidd and Mr. Butcher.

The choice of operation lies between left inguinal and left lumbar, transverse right inguinal and right lumbar.

The operation selected can be contemplated with a view to various results—viz., as in non-malignant ulceration of the bowel, in distension associated with atony, in recto-vesical fistulæ, and in volvulus of the sigmoid flexure, as finally restoring the patient to health, and thus being curative, or palliative, when relieving suffering and prolonging life, by preventing irritating fæcal discharges from passing over malignant ulcerated surfaces; and lastly, the operation may be necessary to avert impending death caused by obstruction of the large intestine, commonly cancer of the rectum, unless, indeed, there may be obvious reasons why such operative interference would be contra-indicated.

Numerous are the conditions requiring colotomy, or perhaps a better term would be colostomy. In the foreground I would place cancer of the rectum, and pain consequent upon cancer of the lower bowel, congenital deformity, narrowing of the gut by tubercular or syphilitic disease, bleeding diarrhœa, stricture, and cancerous prostate.

Among the conditions requiring colotomy, as before stated, I now place cancer of the rectum amongst the foremost, especially those cases in which a radical cure,

by excision of the rectum, is subsequently contemplated; unless, indeed, in those very limited malignant examples sometimes seen close to and around the anal margin, or in extensive cases comparatively free from pain and irritation. For my observation of my own cases and others is, that better results are more likely to follow excision of the rectum when the diseased lower bowel had, prior to and after operation, complete and absolute rest. For what can be more trying than an operation performed on a patient with rectal disease, sleepless from pain, with an exhausted nervous system, and harassed with diarrhœa—all consequent upon this malady; when possible, the superadded inflammatory action of irritation is palliated and reduced in other parts affected with malignant disease, previously to operation; the same rule should apply to the rectum. Furthermore, after colotomy the disease in the rectum occasionally becomes quiescent and atrophy follows from disuse; there are at least two pathological specimens verifying this atrophic condition of the rectum years after colotomy. This rare atrophy appeared to have been produced in a lady I subsequently examined who had been colotomised for rectal epithelioma by Dr. Kidd, whom I assisted at the operation.

There are some who contend that the operation of colotomy should be never performed, as the patient is an object of disgust to himself and unacceptable to others. This is not so, as at present I have under observation persons who have been colotomised, who enjoy entertainments, and are not found to be objectionable.

I certainly agree with those who hold that when the nature of the operation has been fully explained the patient should decide, but I also hold that when it is known by the attendant the relief and prolongation of life that may be afforded, it is his duty to urge an operation, even though

the patient's condition might be repulsive to others. Occasionally no persuasion or argument will induce a patient to submit, even when he is aware of the happiest results colotomy can afford. This occurred to myself in a case of obstruction I was called to see in the County Kerry some few years ago; the patient declining to be operated upon, preferring to endure his suffering and meet the inevitable.

The selection of operation is a matter for consideration. In cases of obstruction with considerable distension, I should prefer lumbar colotomy; the so-called danger of opening the peritoneum is lessened; when there is distension intestine other than the colon is not likely to protrude with this selection. When there is not much distension, although obstruction of a short period, the bowel need not be opened immediately—the surgeon may select for himself; I would then select the inguinal operation.

Again, I would perform left inguinal colotomy to relieve lower bowel pain or hæmorrhage. My experience teaches me to believe that inguinal colotomy is almost always preferable, a better spur is procured, there is more certainty of an artificial anus being formed and less chance of fæcal fistula, besides the convenience of situation. The transverse, right inguinal and right lumbar are not always easy to select from, taking it for granted that it is desirable to colotomise as near to the rectum as possible, and that colotomy is practised above and close to the lesion, which is more readily discoverable when a tumour or localised pain, or distension is present. When not discoverable, exploration above the umbilicus will be necessary to localise, and when uncertainty is cleared up, then an operation near to and above the impediment can be performed; but if by exploring definite information cannot be obtained, I would propose right inguinal colotomy. The

length of the mesentery affords an effective spur in transverse; this is not procurable in right inguinal colotomy.

Here it is convenient to review some of the advantages claimed for inguinal colotomy, as also for lumbar. The advocates of the former claim that the operation is easier, that the diagnosis in doubtful cases can be tested before the bowel is opened, that the small cannot be mistaken for the large intestine, that abnormalities of the colon do not cause abandonment of the operation, that the bowel can be without difficulty drawn to the wound and fixed with facility, that there is not such frequency of prolapse of the gut as in lumbar, that the anterior operation is more convenient to the patient and for the adjustment of pads. On the other hand, the advocates for the lumbar operation endeavour to refute many of the above-claimed advantages, admirably discussed in the *British Medical Journal* by Mr. Bryant.

I have stated that in much distension the peritoneum is not so likely to be opened in lumbar colotomy—I say less likely, for those who have taken the trouble, as I have done, to examine the peritoneal reflexion of the colon, will find that in a very large percentage not any part of the intestine is uncovered by peritoneum. This has been alluded to by Allingham and others, and when there is long mesentery, there is, in fact, not any part of the gut free of peritoneum; and then the probability is, that from the position of the patient during the operation, the descending colon may have fallen towards the right side, causing difficulty in discovering the gut.

Those who assert that the longitudinal bands and appendices epiploicæ, can be observed without opening the peritoneum must have cut through the peritoneum unknowingly.

Taking into consideration that in a large percentage of

lumbar colotomies the peritoneum must be opened, the descending colon may get displaced towards the other side, the cellular and peritoneal inflammation I have known to follow this operation, that it is almost impossible to make as perfect a spur, together with the abnormal and varied positions of the colon—my experience teaches me that anterior colotomy is a preferable and a safer operation, although by no means excluding entirely the lumbar operation, which has its own place in surgery. In lumbar colotomy I practically follow the laws of Allingham—an incision half an inch posterior and immediately between the anterior superior and posterior superior spines of the ilium, and midway between the last rib and the crest of the ilium. My examination led me to conclude that at least one inch posterior and midway between the spines reaches the colon more directly. I prefer the oblique incision of Mr. Bryant to the transverse of Amussat, although I have practised both, as also the vertical incision of Callisen.

In inguinal colotomy I make a short perpendicular incision in the groin, about three inches long, external to the epigastric artery. A scalpel, scissors, a few hæmostatic forceps, a dissecting forceps, needles, and sutures are required, and a director. This last, sometimes required, should be at hand. As soon as the peritoneum is opened the omentum generally appears. The peritoneal wound may now be enlarged, and the divided peritoneum having been seized with forceps, is held in position until sutured to the skin by carbolised silk; this lessens the risk of the oozing from the cut surfaces entering the peritoneal cavity. A sponge which had been previously placed in the cavity to prevent intestinal protrusion is now withdrawn, when the sigmoid flexure generally appears at the wound, recognised without difficulty by the appendices epiploicæ and bands; but should the small intestine or omentum appear at the wound, as occasionally occurs, then by passing the fore-

finger into the abdomen on to the iliacus muscle, the large intestine, as a rule, can be felt, or if not, further search may be made towards the sacrum, and the rectum found and traced up; or the forefinger may be passed upwards and the descending colon followed. The thickness of the large intestine will also be a guide. It is only with an unusually long mesentery that there is any delay in finding the large intestine.

The gut having been brought through the wound, a sufficient portion of the intestine, with a moderately long mesentery, is obtained. Through the mesentery I pass a needle, armed with a silk ligature which is sutured to the parietal peritoneum on each side, taking care that the gut is well protruded through the wound. In cases of a moderately long mesentery (indeed in all cases where there is sufficient mesentery to admit of a good spur), I also suture the mesentery to the parietal peritoneum, about two inches from the protruded portion of gut at the lower or distal end. This is a most important step in the operation, for it prevents subsequent procidentia. Two or three points of suture passed through the serous and muscular coats of the intestine and the edge of the parietal peritoneum where it joins the skin, complete the operation. The gut is covered with iodoform gauze and sal alembroth wool, confined by a bandage; in from ten to twelve hours the gut is completely adherent, and can be punctured if much distension is present, or for the passage of fæces. If there are not indications for early opening, and should the patient be comfortable it need not be opened for two or three days, or even longer, which can be done with a scissors from above downwards. In addition to the above, and when there is a very limited mesentery, I introduce through an aperture made in the mesentery and below the sutures already mentioned, a sterilised elastic bougie, which, crossing the wound, lies upon the abdominal wall—

this materially assists in the formation of a more perfect spur, but is quite unnecessary when the mesentery is long, as the distal sutures would be in a very short mesentery.

Right inguinal colotomy is performed on the same lines as the left inguinal. The cæcum or the part of the ascending colon nearest to it should be opened. Transverse colotomy is not an operation of frequency; I have only performed the operation upon one occasion, which shall be described.

Many years ago I performed left inguinal colotomy. The subject was an infant suffering from atresia ani vesicalis; the family were patients of the late Dr. Wyse. The case was seen first by Mr. Butcher, who advised the operation. Amongst those present at the operation were Dr. W. Corry, Mr. Blyth, and Dr. Charlton. Liquid feculent matter as well as flatus, passed through the urethra, the abdomen was much distended and tympanitic, and in the left inguinal region there could not only be felt, but perceived, a considerable swelling. There was not any difficulty in making a diagnosis; chloroform having been administered, I made an incision parallel to and above, Poupart's ligament. The peritoneum having been opened, the semi-liquid contents of the intestine were pressed upwards to empty the gut, which was held by an assistant and kept as empty as possible. Two sutures, one above, the other below, transfixed the intestine, being also passed through the skin; the intestine was then opened, the sutures pulled up and divided and subsequently tied, and two additional sutures were introduced, one upon each side between those already alluded to. The wound was carefully cleansed, and lint, soaked in oil, placed over the opening in the bowel, together with a pad over the entire; a roller bandage was lightly applied. The boy is now alive and well, about twenty-one years of age; the distal opening of the bowel has become obliterated; through the upper or artificial opening the natural dis-

charge is evacuated; there is considerable sphincter-like action and contractile power round the orifice.

Case I.—Mrs. M., aged forty-three, related that for nearly two years she had suffered from constipation, latterly of the most obstinate form. She had been treated by purgatives, which, up to the period of her admission into Mespil Private Hospital, had relieved her. She suffered severely from pain over her abdomen, and had occasionally afterwards almost continuous sickness, which was never fæcal; her tongue was furred, her pulse frequent and with little volume; she complained of pain, more on the right side of the abdomen which was considerably distended. The result of a rectal examination was negative; no growth was ascertained, nothing was elicited by percussion, and the only evidence of where the site of the obstruction lay was that a wave, probably of fluid, could be felt traversing the ascending colon and stopping about the splenic flexure and again receding. No flatus came by the bowel or through an enema tube passed up the rectum, one end of the tube being in a basin of water.

The only treatment adopted was abdominal fomentations and opiates. Dr. Kidd was to have seen this patient with me, but had to leave town. I had the advantage of the opinion of Dr. Gordon, who was present at the operation. Upon the day following her admission, the 3rd of November, chloroform was administered by Dr. Harley. Above the umbilicus I made an incision about two inches long, which I subsequently extended to about five inches. It is impossible to explore and search the abdominal cavity satisfactorily when no growth is felt, through a limited incision—it is only wasting time. I passed my hand along the descending colon, which was flaccid and undistended, but quite free and movable, then along the transverse and ascending colon, which was greatly distended; a constriction and hardness could be felt a little below the splenic flexure, which decided the transverse operation. The posterior part of the sheath of the rectus muscle formed by the internal oblique was incised. The gut was pulled through the exploratory wound at the upper part, to which the parietal peritoneum had first been sutured, to the marginal skin. I passed a suture through the mesentery as described in left inguinal, and two points of suture traversed the muscular coat of the intestine and parietal peritoneum, which completed the operation, except the sufficient closing of the exploratory incision. Boric powder was applied and

iodoform gauze, the closed surface of the incision having been previously washed with alcohol, a soft wool pad and a light roller bandage finished the dressing. Hypodermic injections of morphia were administered, the patient had ice to suck, and small quantities of iced milk and water to allay thirst. This patient was permitted from choice to have ice; but in the majority of my cases I have found mouthfuls of warm water more effectual. The morphia injections did not suit her, so I prescribed phenacitine every three hours; it has the advantage of not interfering with the action of the intestines. Twenty hours after the operation I deemed it expedient to puncture the intestine with a fine, grooved needle; there was a free escape of flatus; a small piece of wool soaked in collodion was promptly applied to the needle puncture, for I feared lest the large wound might in any way get contaminated, although its surface had been carefully guarded and painted with iodoform and collodion. The following day her temperature was normal, her pulse was fuller and less frequent. I made a small opening into the gut with a scissors; forty-four hours after the operation volumes of flatus escaped, and about 2 oz. of fluid feculent matter. The progress of the case was without interruption; the lady is alive and well—fat, ruddy and healthy, nearly three years since the operation has been performed.

Case II.—A tall, handsome, well-built, active gentleman, about forty-two years of age, consulted a physician for about one year, and came frequently to Dublin for the purpose. He stated he was told that he was suffering from catarrh of the intestines. Suddenly he got hæmorrhage from his rectum and was sent by the physician to a surgeon, who, amongst other things, passed a silver catheter into his bladder. Almost immediately upon the withdrawal of the catheter he passed wind through the urethra, and some sanious, bloody fæcal fluid followed, he drew attention to this, but his condition was not regarded as serious, and he returned to the country. Shortly after he consulted me, in September, 1894. There was not any difficulty in discovering a large malignant tumour, which occupied the hypogastric region and extended towards the left iliac region; the growth was somewhat larger than an orange, and could be easily felt in the rectum. He was in a deplorable condition and had much bladder irritation; he had lost weight, and was troubled with frequent diarrhœa and calls to micturate. I colotomised him in the left inguinal region. Dr. Harley administered the chloroform, and I had the advantage of Dr. Kidd's assistance. The patient had

a very short mesentery, so much so there was difficulty in obtaining sufficient gut. In his case I used the bougie before alluded to, which considerably assisted in the formation of a good spur. The bowel was opened three days after the primary operation, the fæcal discharge and flatus ceased to pass through the urethra, he gained weight and strength and was very comfortable. Eight months after the operation he was well; subsequently he had an attack of hæmorrhage and died about two months ago.

Case III.—A gentleman farmer, aged sixty-six years, consulted me in February, 1893, for a lump (to use his own phraseology) in his lower bowel, which caused him much inconvenience when his bowels were moving, and a considerable amount of straining and inability to "empty his bowels;" he had not any difficulty in passing water and looked fairly healthy, although he stated that he felt himself failing. Upon digital examination a large tumour could be felt nearly blocking up the rectal canal and extending beyond the length of my finger. It was easy to determine that the growth was prostatic and malignant. Being aware of cases of the kind reported by Molière and others, I advised colotomy, having first explained to the patient that the straining and labour when the bowels were being acted upon would probably cause the tumour to increase more rapidly and break down, and likewise save him from pain and distress. He consented to my recommendation; I performed left inguinal colotomy; Dr. Hadden, Dr. Harley, and Mr. Starkey were present. The mesentery was a very long one and a most efficient spur and artificial anus were formed. The gut was opened forty-eight hours after the primary operation; the patient progressed rapidly, and expressed himself much gratified by the relief obtained from the operation. I saw him two months later; his general condition had much improved, and his appearance was that of a healthy man. He left Ireland to visit some friends in France six months after the operation; at the time, I am informed, he was in good health and had not any discomfort from the growth. I heard he died seven months later, during his residence in France, thirteen months after the operation, having had two or three attacks of hæmorrhage.

Case IV.—The following case, the details of which I shall briefly relate, although not colotomised, bears upon this communication. I was called to the Co. Wexford to see a lady, aged forty, who had suffered for a week from intestinal obstruction. She was very much

distended and had occasional vomiting, and had suffered acute pain just below the umbilicus. It was quite impossible to determine where the seat of the obstruction was; palpation and percussion did not afford any clue, save that below the umbilicus, where she had suffered acute pain, there was a circumscribed area of dulness. The patient having been chloroformed by Dr. Boyd, I made an incision over the area of dulness, about two inches long and finally extended it. I was ably assisted by the local medical gentlemen. When the peritoneum was opened a shrivelled distended piece of small intestine, about two inches long, came into view; when caught between my fingers it separated, leaving the ends of the small intestine, between which it lay, sealed up and closed. I sutured the parietal peritoneum to the wound and the two sealed portions of intestine, which I opened by incising the closed parts; flatus passed out from both ends of the intestine; the patient lived for forty to fifty hours. I was informed that she died of heart failure.

For congenital malformation in infants, impossible to be treated through the peritoneum, I have performed four inguinal colotomies, one lumbar, the result being in accordance with the statistics of this operation on children—one recovery, four unfavourable; one transverse colotomy; recovery, case recorded; five left lumbar, four operated on for extensive cancer of the rectum with glandular complications; one case lived for two years and two months, two died within a year and a half, one died in seven months, one lived nearly four years and one month after a subsequent excision of the rectum, and died from a return of the disease three and a half years after the rectum was operated on. In all these cases, except one, the opening of the gut was delayed. Of nine inguinal colotomies, one was right inguinal performed upon a gentleman between sixty and seventy years of age. Five years previously I had excised a large portion of his tongue for cancer. When I was called to see him he had suffered from intestinal obstruction for nearly ten days. There was much meteorism, the gut was opened immediately; he died fifty hours after the operation

of collapse. Dr. Gordon, Dr. Swan, Mr. O'Grady, and Dr. Harley kindly assisted me. Another was for syphilitic ulceration of the rectum, known to be alive six years after; two for ulceration, one with hæmorrhage and pain in addition. These cases are alive now three years and three and a half years respectively. Opening of the bowel was delayed; one for malignant tumour situated in umbilical and left iliac regions, before recorded; one for prostatic malignant disease, recorded; three cases for very extensive cancer of the rectum, one causing complete obstruction. One lived four months, much relieved, bowel opened immediately; one lived fifteen months with comparative comfort, the third case lived nine months. In the two latter, the opening of the bowel was delayed.

I shall not trouble the Section with colotomy statistics. The most complete statistics will be found in the *American Journal of Medical Science*, October, 1889. Suffice it to say the mortality after all operations for malignant disease was only a little over 30 per cent.

Amussat's method shows a mortality of 25 per cent., Littre's 45 per cent. Of 52 operations for imperforate anus more than half died; of 40 operations for obstruction one-half died, Amussat's operation giving the most favourable results. Statistics cannot possibly fairly estimate the mortality; a history of each particular case would be required, for many patients have been colotomised long after the time for such operations had passed. Not taking into consideration the cases operated upon for deformity, nearly always fatal amongst the number I have selected to record, there has only been one death attributable to colotomy (fifty hours after), and performed for acute obstruction.

In conclusion, I may state that all the patients I have colotomised, who were capable of giving an opinion, expressed themselves as much relieved and comforted by

the results following this operation; and it is my firm conviction that, in the majority of the cases not relieved, the fault lies at the door of the operator, for should the result following colotomy be a fæcal fistula and not an artificial anus what benefit can follow? The fæcal discharge still passes over the diseased surface, the irritation and pain continuing as before the operation, in addition to the discomfort of the artificial opening; this is caused by an insufficient spur. True, there are some cases where there is such a short mesentery, or no mesentery at all, that it is impossible to have a sufficient spur; but such cases are not frequently met with. I cannot too strongly condemn the procedure of dividing the intestine and suturing the lower end and dropping it into the abdominal cavity, for fæcal matter may remain in the lower divided portion, and cause ulceration and peritonitis: and should there be twisting of the gut (and not observed), the superior portion may be sutured instead of the lower. I also deprecate pins passed through the gut to fix it; flatus and fæces may follow the course of the punctures; procidentia may occur—one of the greatest discomforts after colotomy; but the precautions which I have recommended of suturing the mesentery at the distal end of the gut will prevent it, and is far preferable to pulling out the gut above and below until the mesentery is "taut," and subsequently removing the gut external to the abdomen, which procedure may be followed by hæmorrhage, and in some cases the removal of the superabundant gut is inadmissible. I have never found it necessary to use an insufflator. I prefer chloroform as an anæsthetic. Some years ago, at the old Surgical Society, I recorded my experience in the reduction of hip dislocations, that with chloroform there was more muscular relaxation, an important factor when searching the abdominal cavity for obscure obstruction. It is advisable to have the bowels acting soon after the

gut has been opened, and when the bowels begin to act the wound may be dressed with carbolised oil or thiocamf, as recommended by Dr. Duffey. Time does not permit me to enter further upon the after-treatment of colotomy, or upon the application of pads or trusses, or upon the curative treatment of artificial anus.

There is indeed much, very much, more to say upon the all-important subject of colotomy, to which I would draw attention were it not that I feel I have already occupied more than the allotted time.

Mr. Henry Gray Croly read a paper on left lumbar colotomy, and stated that he had, during the past twelve years, performed that operation eighteen times. With one exception, the operations were performed for cancer recti, and the cases were unsuited for excision; no death occurred from the operation. Two of the patients (notwithstanding careful antiseptic precautions) were attacked with erysipelatous inflammation and septic or erysipelatous pneumonia, but recovered. In each case great relief was afforded to the sufferers. The patients lived from a few months to over two years; one man worked for nearly two years after the operation as a Corporation labourer. All the patients got fat and expressed themselves gratefully for the relief afforded to their sufferings. He did not intend to compare the lumbar operation with the now more fashionable inguinal operation, but thought it his duty to place on record brief notes of the cases. He adopted the oblique incision of Bryant, and in his hospital practice marks out the crest of the ilium—last rib and erector spinor muscles; he has invariably found the colon half an inch behind the centre of the crest. He opened the intestine at once and made an opening not longer than half an inch, and had thus prevented the prolapsus which occurred in some of his early cases. The following difficulties were encountered in performing the lumbar operation—viz., on one occasion the gut was collapsed and the kidney presented, but the intestine was soon found; in two cases the distance between the crest of the ilium and last rib was very short, and consequently the wound was deep. He had found considerable help by having the intestine inflated by means of Higginson's syringe and by rolling the patient backwards when the gut pre-

sented. He fed his patients after operation on soups, eggs and milk, and had the lower bowel carefully washed out with antiseptics. Each of the cases came to him as "pile cases," and one patient was under treatment for dysentery in a clinical hospital for several months, and no digital or other examination had been made of the rectum. He claimed for lumbar colotomy its extra-peritoneal advantage, and he considers the position of the artificial anus in the loin less disgusting to the patient than the inguinal.

The PRESIDENT expressed surprise that lumbar colotomy should be advocated at the present time. Mr. Bryant's name had been brought forward as supporting it. He did not agree with Mr. Bryant's views with regard to lumbar colotomy. Mr. Wheeler sutured the peritoneum to the skin. He found that a much better anus was formed if this was not done, but the bowel was allowed to join the muscular and fibrous coat as well. He thought that in some cases the gut might be cut across, and so any chance of fæcal matter passing into the distal part prevented.

SIR WILLIAM STOKES said he had listened with great interest to the two communications. The more experience he had of lumbar colotomy the more highly did he think of it. Still he did not think ill of the anterior operation. Through Mr. Croly's courtesy he had seen some of his operations, and he said that the photographs well represented the excellent results obtained. One advantage to the lumbar over the inguinal operation, pointed out by Mr. Henry Morris, was that, as the patient had to lie on his back for a considerable period after the operation, there was a better chance of fæcal drainage. In the anterior operation hard masses of fæces frequently blocked the opening when it was made low down. In the lumbar operation the gut could be opened without opening the peritoneum in a large proportion of cases. The anterior operation had, however, the advantage of the greater facility and comfort by which the toilet of the patient could be performed.

DR. FITZGIBBON spoke from personal observation of the invariable relief from pain and the prolongation of life which followed lumbar colotomy. He had also performed two inguinal colotomies, one of which was an illustration of the disadvantage of inguinal as compared with lumbar colotomy. The subject was a woman admitted to the Westmoreland Lock Hospital in July, 1892, suffering from a syphiloma of the rectum, which occluded it. There was an artificial anus opening into the vagina. She objected at the time to a colotomy, so he enlarged the opening into the vagina and relieved the symptoms of obstruction from which she

was suffering. She left but returned again, as the syphiloma had extended into the vagina. He performed a left inguinal colotomy. She completely recovered. She returned in October, 1893, complaining of prolapse of the artificial anus, which could not be kept returned. She was pregnant at the time. After delivery the prolapse could not be kept back.

Mr. Kendal Franks said that, with regard to the advantage of dependent drainage in lumbar colotomy, mentioned by Sir William Stokes, he did not see why a patient operated on by the inguinal method should not be turned on his side. One of the great reasons why inguinal colotomy had met with so much favour was the greater ease with which the sigmoid flexure could be found, compared with the finding of the descending colon in the lumbar operation; also, it was not always easy to know what piece of intestine presented in lumbar colotomy. The extended use of excision of the rectum, especially by the trans-sacral method, was gradually turning colotomy out of the field, so that colotomy, he believed, would soon become a very exceptional procedure.

Mr. M'Ardle agreed with the President's views with regard to lumbar colotomy. If the anterior operation was carried out fairly high up, where the bowel was readiest of access, there should be no danger. He also agreed with the President that the peritoneum should not be sutured to the skin. He passed a supporting needle or peg beneath the bowel, but did not pass any silk through the bowel. He did not use any sutures. The operation lasted only a very short time, and he thought this of great importance.

Mr. Wheeler, replying, said that in his opinion the anterior operation is preferable and safer, but that in some cases the lumbar operation should be done. He thought that cutting the gut and returning it into the abdomen was bad surgery, as the superior end might have been sutured instead of the inferior, and symptoms of strangulation might follow from fæces remaining in the distal portion, or the foreign body might ulcerate through. He had found that suturing the mesentery at the distal end kept the prolapse in abeyance. He did not think a catheter under the bowel formed as good a spur as suturing the peritoneum.

Mr. Croly said he believed the mortality was greater in inguinal than in lumbar colotomy. He thought that the lumbar colotomy should not be shirked because it was a more difficult operation. In his paper he had not intended to compare inguinal and lumbar colotomy.

CASE OF EXCISION OF THE RIGHT HALF OF THE LARYNX FOR CANCER.

By ROBERT H. WOODS, M.B., F.R.C.S.;
Throat Surgeon to the Richmond Hospital, Dublin.

[Read in the Surgical Section, December 6, 1895.]

THE interest attaching to radical operation for malignant disease of the throat, and my belief that this is the first case of its kind in Ireland, will, I trust, be deemed sufficient apology for this communication.

J. M'Q., aged thirty-two, by trade a riveter, came to the throat department of the Richmond Hospital, on March 20th, 1895, complaining of difficulty in swallowing and pain in the throat of six weeks' duration. The pain at first was slight, but became progressively worse, and during the last few days was so acute during deglutition, that he voluntarily starved himself rather than endure the agony of swallowing solids; liquids, however, could be taken with comparative ease. He had never previously been ill, and there was no history of syphilis.

His family history was unimportant, except that his father died of cancer of the neck.

On examination, a hard fixed nodule, about the size of a large bean, was seen and felt at the superior border of the right ala of the thyroid cartilage. The laryngoscope showed that, corresponding to this nodule, the larynx contained a large tumour; this tumour involved the right ary-epiglottic fold in its whole extent, touched the posterior pharyngeal wall behind, and so encroached on the lumen of the larynx internally as to diminish the air space by more than half. The larynx was pushed bodily forwards, so that a considerable interval lay between the right arytænoid cartilage and the vertebral column; here there was situated, just above the patent opening of the œsophagus, a necrotic patch, which lent a gangrenous fœtor to the breath, and perhaps caused some of his pain. The glands under the sterno-mastoid muscle at the level of the cricoid cartilage were enlarged. By passing the fore-finger

of the left hand over the base of the tongue the tumour could be felt to be hard and firm. The lungs were healthy.

The case was diagnosed to be one of cancer, but in order to place the matter beyond doubt, and save the valuable time necessary to differentiate by drugs between it and syphilis, a small piece of the growth was avulsed by means of a sharp hollow forceps, and handed to Dr. H. C. Earl, who submitted it to microscopic examination, and on March 28th reported it to be a carcinoma.

The disease was evidently very far advanced, and must have been established months before it attracted his attention; indeed it is likely that he only suspected mischief when the pain of the ulceration disturbed his quiet.

The question of interference was an anxious one, but, after consultation with my surgical colleagues, it was agreed that it would be better to give him the chance of operation rather than leave him to wait in pain and starvation for inevitable death. Excision was therefore recommended. The dangers and difficulties of the operation, as well as its after effects and the rarity of permanent cure, were fully and fairly laid before the patient, who, without hesitation, decided on having it performed. He was told that the whole larynx might have to be removed, and he not only agreed to leave the matter entirely in my hands but expressed the wish that I should take away too much rather than too little.

The operation was performed on April 1st. One-sixth of a grain of morphia was injected subcutaneously and the patient chloroformed with a Skinner's inhaler; a sand-bag was placed at the back of the neck and the head extended. Before narcosis was induced he struggled a good deal, and his breathing sounded as if there was some obstruction in the larynx. An incision was carried along the middle line from the hyoid bone to the suprasternal notch, the trachea was bared, and high tracheotomy performed. A

transverse cut was next made from the upper end of the mesial wound outwards, and to the right parallel to the hyoid bone and close below it. The thyroid cartilage was bared and, with the subjacent mucous membrane, cut in the middle line with a stout scissors. The thyrohyoid membrane was similarly treated. The wings of the thyroid cartilage were dilated and the larynx inspected. The growth was confined to the right side and nearly filled the larynx; there was œdema of the left ary-epiglottic fold, due apparently to infection from the necrotic surface behind, and explaining the dyspnœa during the early administration of the chloroform. A few drops of a 20 per cent. cocaïn solution were brushed on the mucous membrane to diminish its sensibility and allow greater freedom of manipulation. A strip of sponge, with string attached, was introduced into the larynx and packed round the upper part of the tracheotomy tube, to which a Trendelenburg's apparatus was attached for the administration of the chloroform during the subsequent steps of the operation.

The skin and subcutaneous tissues were stripped from the triangle marked by the mesial and horizontal incisions as far back as the sterno-mastoid. The right crico-thyroid membrane and adjacent structures were cut through, the crico-arytænoid joint opened, and the right half of the larynx thus freed from its connections with the cricoid cartilage. The thyrohyoid membrane was next cut close to the hyoid bone, and the epiglottis snipped along the middle line; its right half was brought into the wound and drawn forcibly downwards, while the tissues were cut between the horizontal wound externally and the vallecula within. The great cornu of the hyoid bone was found to run so close to the tumour that it was removed, and the height to which the dissection had to be carried involved, as we afterwards found, division of the hypoglossal nerve; the lingual artery, however, escaped

The mass was then detached posteriorly from the prevertebral muscles, the bag of the pharynx being divided to the left—the healthy side—of the middle line, the attachments to the carotid sheath were cautiously severed, and finally a pedicle by which it was held to the base of the tongue was snipped through and the mass removed. The hæmorrhage was occasionally pretty sharp, but at each step the bleeding vessels were caught and ligatured; the quantity of blood lost altogether was not great.

Infected glands were then sought for. A number of them were followed towards the angle of the jaw and carefully dissected out. A second chain, underlying the sterno-mastoid at and below the level of the cricoid cartilage, was next isolated and extracted, great care being necessary to avoid wounding the internal jugular vein on which they lay and which they to some extent clothed. The whole wound was next examined, but nothing suspicious either to the eye or touch remained. The carotid artery and internal jugular vein were found to have been exposed for three inches of their course.

The raw surfaces were sponged with tr. benzoin co. I am aware that the classical antiseptic and hæmostatic for excision of the larynx is chloride of zinc, but I consider any soluble antiseptic too temporary and unreliable in its action when used in a situation where the secretion cannot fail to carry it away within a very short time. Friar's balsam, on the contrary, forms an adhesive coating to the tissues, and that it effectually prevented infection in this case is sufficiently evidenced by the temperature chart.

The horizontal wound was next sutured, the mesial one was left open; a No. 14 red rubber œsophageal tube was pushed into the stomach and sutured to the skin at the upper end of the central incision. The cavity of the larynx and lower part of the pharynx were plugged with iodoform

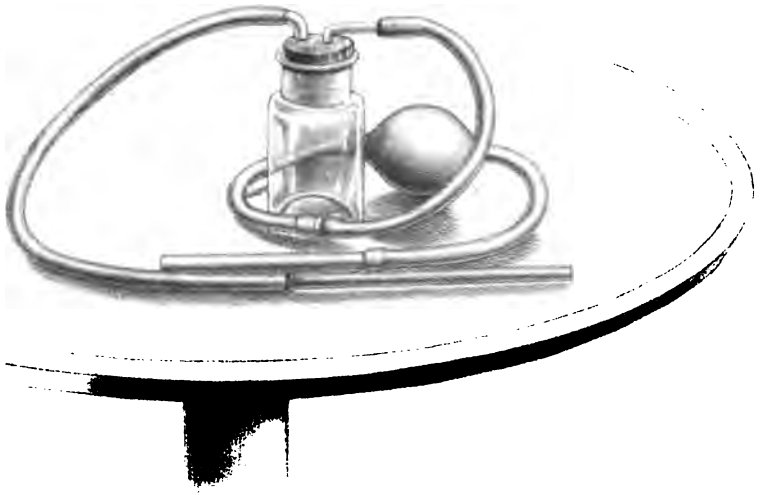
gauze, and the patient put to bed breathing comfortably through the tracheotomy tube.

The patient took the chloroform badly from first to last. Three times the respiration ceased without mechanical cause, the pulse, which was carefully watched, slowed down, and the patient became pallid and ashy. Patients after tracheotomy develop a condition of physiological apnœa, the chief cause of which is diminution in the length, and consequently of the capacity, of the passage between the lungs and the outer air. But the condition of the patient on these three occasions was quite different from the apnœa of tracheotomy, as evidenced by the failure of the heart and the change in colour. On each occasion the operation had to be stopped and artificial respiration started. After the third time the chloroform was suspended and the operation finished while the patient lay in a lethargic but not quite unconscious condition. He, however, felt no very acute pain. The tumour was examined microscopically by Dr. Earl and found to be a squamous epithelioma.

During the days immediately following morphia was freely given by the skin, with the double object of lessening the shock and avoiding bronchitis or pneumonia—the commonest causes of death after this operation. No steam apparatus was used; the temperature of the room was carefully regulated and draughts excluded.

At the end of the first twenty-four hours he had a reactionary temperature of 100·8° F., and this was the highest point it ever reached. He had no pain, very little tenderness, and had not vomited. He was given a little milk through the tube, which he bore well. The wound was dressed, the plug of gauze being removed, and a powder of iodoform and boric acid insufflated before a fresh one was replaced.

On the third day the tracheotomy tube was dispensed with; the stitches were taken out of the horizontal wound,



Mr R.H. WOODS' BLOOD-SUCKER.

which had healed by first intention. The stomach tube was removed and cleansed, and the gauze plug withdrawn and not replaced. On the fourth day, his bowels not having been moved since the operation, a saline draught was administered. He shortly afterwards vomited frequently, and complained of an "icy cold pain" in the pit of the stomach. Withdrawal of the stomach tube relieved this pain. The tube was left out, and not re-introduced except at meal times, when it was passed into the œsophagus through the wound in the neck.

From this time his recovery was uninterrupted. He was allowed up on the tenth day, and was able to enjoy a pipe on the fourteenth. There was a good deal of secretion from the granulating surface, and saliva trickled into the throat. A convenient apparatus for their removal was made from a bottle with two glass tubes passed through the cork. To one tube an enema syringe was attached "tail on," so that when worked it pumped air *from* the bottle; and to the other was fixed a rubber tube, to which a long brass nozzle of $\frac{1}{4}$-inch diameter could, if necessary, be fixed. By putting the nozzle to the required spot the secretion and saliva could be removed rapidly by working the syringe, the bottle acting as a reservoir. This little instrument was first designed by me for removing blood and saliva in cleft-palate operations in the "head down" position. I have used it also with very great advantage in laryngofissure and pharyngotomy. By its means blood can be removed in a tenth of the time and with less than a tenth of the trouble taken in sponging, and perhaps, most important of all, without exciting any reflex spasm in the patient.

The patient swallowed corn flour on the twenty-eighth day, and did not require to have the tube passed after that time.

Seven weeks after the operation there was slight inspiratory dyspnœa, due to cicatricial narrowing of the throat.

This was successfully treated by the daily passage of Schrötter's stenosis tubes.

From this time he was perfectly well. There was absolutely no pain at any time; he slept well, and could swallow meat and drink without difficulty. It was no uncommon thing to see him drink a pint of milk without taking the mug from his mouth. He spoke with a hoarse but very fair voice, making himself heard across an ordinary room without effort. He expressed himself as well pleased with the operation, and said that any inconvenience he had had to suffer was as nothing when compared with the relief it had afforded him. He spent over three weeks at the Convalescent Home, Stillorgan, and was exhibited both at the Surgical Section of the Royal Academy of Medicine and the Laryngological Section of the British Medical Association in London.

He was examined at regular intervals for any sign of recurrence, with negative results however, until early in August, when an indurated lymphatic gland was felt low down on the right side of the neck, behind the sternal head of the sterno-mastoid muscle. Its nature was only too obvious. Removal was recommended, and agreed to by the patient with a cheerfulness and alacrity that characterised him from the very first, and the operation was undertaken on August 21st.

In view of the difficulty encountered in keeping him alive under chloroform on the former occasion, it was decided to use cocaïn, and three-quarters of a grain was accordingly injected under the skin through two punctures at the site of incision. The gland was exposed and found lying on the internal jugular vein, just at its junction with the subclavian. The dissection was conducted most cautiously, and the danger of venous canalisation kept in view—an accident once seen by the writer in a fatal case under the hand of the late Professor Billroth. The gland was more than half isolated when a

membrane was punctured, and air heard to rush through the hole with a characteristic sw—— sound. The finger was at once placed on the puncture, and a pause made to watch if any effect followed. There was no change in the patient's condition, so that the quantity of air which had entered the vein must have been too small to cause distress, or else the breach must have been made into the cervical pleura. Either was quite possible in the situation, but the latter was thought more probable. To settle the doubt the patient was directed to cough. During the effort the finger was removed, when a jet of venous blood showed the true state of affairs. An effort was made to secure the puncture, so as to admit of a further attempt to isolate the gland, though the prospect of its possibility was not an encouraging one, it being by this time pretty certain that the gland was adherent to the wall of the vein. The perforation, however, could not be secured, and the attempt was attended by the entry of more air. The patient immediately developed the characteristic symptoms of canalisation. The face became intensely congested, the lips blue, and the skin covered with a profuse sweat. He felt smothered, and asked to be allowed to sit up. This he was let do. Oxygen was procured at once, and administered during inspiration through a tube in the mouth; the clips were taken from the wound and the edges held firmly together by the fingers, and subsequently clipped by forceps; the wound was then closed and the operation abandoned. The effect of the oxygen was marvellous; the patient's colour at once returned, and his distress ceased. The gas had to be temporarily stopped to allow the apparatus to be adjusted, and he promptly reverted to his former condition, recovering, however, with the re-administration. From its effect on the patient there is little doubt that oxygen is the quickest and best remedy for such cases; and I should recommend that, where there is a risk or certainty of open-

ing veins near the root of the neck, the operator should have a supply of the gas at hand. After the lapse of three-quarters of an hour he had quite recovered, and the oxygen was discontinued. The patient felt quite comfortable that afternoon and the following day, except for some pain in the chest, especially on coughing. He had complained of this, however, before operation.

The cough became more and more distressing, and his condition daily worse, and he died five days later, nearly five months after the excision.

Post-mortem examination.—There was severe inflammation of the whole respiratory tract from the trachea to the bronchioles. The lower lobe of the left lung was of a dusky red colour; and, on squeezing the tissue of the lung, drops of muco-pus exuded on to the cut surface from the air passages. The vein punctured during operation was found to be the internal jugular just as it joined the subclavian The vein was healthy, and no attempt at thrombosis was observable. The site of the first operation in the larynx was perfectly cicatrised, and there was no suspicion of recurrence *in loco*. There were no mediastinal glands engaged. There is every reason to believe that this gland in which recurrence took place was a far-down member of the chain on the carotid sheath dissected out at the first operation; and though, as far as one could tell, it was completely excised, it is now evident that the last and minutest link must have been left behind.

It is a rule with some operators not to touch malignant disease of the larynx when there is any palpable glandular enlargement. I question its wisdom. Secondary involvement of the axillary glands does not now-a-days necessarily contra-indicate operation in cancer of the breast, and, though it may add to its difficulty, need not prevent its success; and I can see no reason why, as our technique becomes more

perfect, the same should not be true of cancer of the larynx. I do not regret having acted contrary to this rule. The man's life was prolonged for some months, and his condition from being one of acute suffering and starvation was rendered tolerable and even enjoyable. It is, perhaps, not too much to say that had the patient presented himself a little earlier, before the cancer had progressed quite so far, the same operation might have removed every trace of that most terrible of diseases.

Mr. Croly thought that filling the wound with water was the best way of preventing canalisation. The difficulty of having oxygen always at hand was very much against its use in cases of emergency, such as of air entering a vein. He was rather sceptical as to the value of oxygen in such cases. He (Mr. Croly) desired to put three questions to Mr. Woods:—1. Why he did not remove the whole larynx? 2. Why he did not use water to prevent canalisation? 3. Why he did not ligate the jugular vein before proceeding to remove the glands adherent to its walls?

Mr. Kendal Franks said Mr. Woods deserved to be congratulated on having described in a very interesting manner the details of an exceptionally brilliant operation. Unfortunately, the result was fatal, but death could not be ascribed to excision of the larynx: it was due to an extension of the disease, for the removal of which the operation had been undertaken. He believed he was right in describing Mr. Woods' case as the first undertaken in this country for the partial removal of the larynx. He had seen, in such a case, ether administered by the rectum, but the patient died within 48 hours from colliquative diarrhœa, and the *post mortem* showed acute inflammation of the whole of the large intestine. The administration of chloroform by means of Trendelenburg's apparatus through an opening in the trachea, as performed by Mr. Woods, was far better. He entirely disagreed with Mr. Croly's view that the whole larynx should have been excised. It made a great difference to the patient whether he lost his whole larynx or only half. Recurrence did not take place in the healthy side, but in the cicatrix, or in the glands of the neck of the affected side. Moreover, partial excision was far from being as dangerous as complete

excision. Mr. Franks asked why Mr. Woods, in the second operation, did not tie the vein above and below, and remove the included portion with the adherent implicated glands.

Mr. Wheeler said that the rectal method of administering anæsthetics was not to be recommended. In one case that he had ether thus administered, the intestines became distended, pressed on the diaphragm, and considerably embarrassed breathing.

Professor Bennett thought that a great source of danger in the administration of ether by the rectum was that the ether was apt to accumulate in the intestines, and therefore the quantity given was not easily controlled.

Mr. Woods, in reply to Mr. Croly, said that his finger introduced into the wound completely controlled the vein, and prevented the further entrance of air, and was much more rapidly effectual than filling the wound with water. The immediate improvement in the condition of the patient after the administration of oxygen, suggested oxygen as perhaps the best remedy for canalisation. The implicated glands were adherent not only to the internal jugular, but also to the subclavian and innominate veins, and the condition of the patient would not permit of such a serious proceeding as ligature and excision of these veins would involve.

SUCCESSFUL OPERATION FOR A CASE OF PULSATING TUMOUR OF THE TEMPORAL REGION OF EIGHTEEN YEARS' STANDING.

By C. YELVERTON PEARSON, M.D., F.R.C.S.E.;
Surgeon to the North Charitable Infirmary and City of Cork Hospital, &c.

[Read in the Section of Surgery, January 10, 1896.]

CASE.—W. A. M., male, aged forty-five, grocer, married, residing in Cork, admitted to North Charitable Infirmary, October 15th, 1894.

Family history.—Father died of liver disease, mother of paralysis; both at an advanced age. Patient has nine children—seven boys and two girls—all healthy. The eldest, a boy of eighteen years; the youngest, a boy of eighteen months.

Previous illness.—Had rheumatic fever when fourteen or fifteen years of age; also had scarlatina and measles. Otherwise healthy up to onset of present affection.

History of present affection.—About eighteen or nineteen years ago got a fall from a horse, and struck his head, but does not remember noticing any special injury at the time. A few months afterwards felt pain in left frontal and temporal region, which was at first of a dull character, but which gradually became more intense, and has continued ever since. Also complained of dizziness and a sensation of fulness in the head on stooping. Also noticed, at a somewhat later period, a swelling of the left temporal region, of a throbbing character, which has increased very gradually. In the course of about two years from the onset the pain, fulness, and dizziness became so great, on stooping forwards, that he was obliged to give up his business. He consulted several doctors, both in Ireland and England, and was treated chiefly by external applications, and advised to take plenty of fresh air. Only one medical man recommended an operation.

About three years ago he presented himself at the out-patient department of the North Infirmary, when he first came under my observation. I investigated his case very carefully at the time, and came to the conclusion that the swelling was of an aneurysmal

character, most likely of traumatic origin, and, probably, associated with some intra-cranial mischief of a vascular nature. A consultation was held at the time with some of my colleagues, and it was not considered advisable to employ operative measures. He was, accordingly, treated with iodide of potassium, bromides, and, subsequently, digitalis, with a slight degree of temporary relief of his most distressing symptoms, but no permanent benefit, and disappeared after a time.

He again presented himself in October, 1894, and described his condition as being so unbearable that he implored to be admitted for operative treatment.

His condition on admission was as follows:—Average height; fairly stout; dark complexion; head proportionately large; appearance healthy; fairly intelligent, and mentally sound; but in depressed spirits on account of his malady. Thoracic and abdominal viscera healthy, and all bodily functions satisfactorily performed; but did not sleep well, owing to pain and throbbing sensation in the head.

Symptoms.—Constant headache and dizziness; cannot stoop forward without feeling giddy, and is unable to look upwards or throw the head backwards without feeling great pain. The pain is chiefly located in the left frontal and anterior temporal region. There was total incapacity for physical or mental occupation.

Physical signs.—There is a uniform swelling of the left temporal region, with perfectly smooth surface, convex, bounded by the temporal ridge and zygoma. The superficial temporal artery, in a much dilated and tortuous condition, can be easily recognised on the surface, as seen in the accompanying photograph. The tumour is soft on palpation, but has distinct pulsation of an expansile character. This pulsation cannot be arrested or even perceptibly affected by digital compression of the temporal artery or simultaneo s compression of the temporal against the zygoma and its branches of distribution above the seat of the swelling. On careful palpation an irregular fissure, about a quarter of an inch in its longest diameter, can be felt near the margin of the swelling, about one inch and a half above and behind the external angular process of the frontal bone, and, probably, situated in the coronal suture. Pulsation cannot be arrested by making firm pressure over this fissure, but is somewhat diminished by making pressure on the tumour itself. The pupils are equal, and respond normally to light. Ophthalmoscopic examination showed the existence of a choked

DR PEARSON ON PULSATING TUMOUR.

F Huth, Lith. Edin.

disc and some optic neuritis on the left side, and increased vascularity of the right fundus.

Differential diagnosis.—The possible pulsating tumours of this region are—Hernia of the brain or its membranes; fungating tumour of the dura mater; pulsating tumour of the bones; malignant growths; and aneurysms. Malignant growths were excluded by the duration of the symptoms and the very gradual increase in size; hernia cerebri and meningocele by the absence of symptoms on pressure, and the very small extent of the fissure in the cranial wall; fungus of the dura mater by the slow progress, the well-defined boundaries of the swelling, and the firmness of the bones beneath; hence the conclusion that the tumour was of an aneurysmal character. The question, however, arose as to the precise nature of the aneurysm. Owing to the smooth surface and anatomically defined margin of the tumour, the absence of diminution in pulsation and size on compressing the temporal and its anastomotic connections, and the existence of intra-cranial symptoms, I formed the opinion that, in all probability, the tumour was a false aneurysm circumscribed by the temporal fascia and muscle with intra-cranial vascular connections, and, therefore, probably supplied by the middle meningeal or deep temporal arteries, or by both.

Owing to the sufferings of the patient, his inability for work, and his own personal solicitations, an operation was determined upon, and was accordingly performed as follows:—The superficial temporal artery was exposed by a small incision immediately above the zygoma, and ligatured. A curved incision was made along the line of the temporal ridge, commencing a little in front of the coronal suture, and terminating below and behind at the posterior root of the zygoma. The integuments were reflected downwards and forwards and the temporal fascia laid bare. The temporal fascia was then carefully divided over the centre of the tumour and reflected, when a highly vascular and convoluted mass was disclosed, bearing considerable resemblance to the living brain and its membranes when the dura mater is removed. On close inspection this mass was found to consist of very large and tortuous thin-walled vessels, which freely anastomosed. An attempt was made to ligature the larger vessels at the circumference, but, after a few trials, this was found to be unsatisfactory, as the walls of the vessels were so thin that they would not bear the pressure of a ligature or forceps, and the hæmorrhage was exceedingly copious. I then deter-

mined, with the consent of Professor O'Sullivan, who rendered me valuable assistance throughout the operation, to attempt its removal with the Paquelin's cautery knife. This I proceeded to do from behind forwards, keeping close to the bone. Very little trace of the temporal muscle was recognisable, its place being occupied by blood-vessels. Numerous small vessels were encountered issuing from apertures in the bones. On reaching the fissure previously referred to the hæmorrhage that took place was truly appalling, and could not be completely controlled by direct pressure combined with pressure on the carotid. One very large and several smaller anastomosing vessels issued through the aperture, which was situated in the coronal suture, about $\frac{3}{4}$-inch above the anterior-inferior angle of the parietal bone. The hæmorrhage was, however, successfully arrested by inserting a cautery in the fissure at a dull-red heat, and maintaining it in that condition for some minutes. The tumour having been removed, and all hæmorrhage having been checked by application of the cautery at all the necessary points, the integuments were replaced and sutured, except where drainage tubes were inserted at the posterior and lower portions of the wound. Careful graduated pressure was applied, especially over the region of the fissure.

My original intention, if found feasible, was to trephine the skull after dealing with the intra-cranial tumour, but, owing to the conditions I have described, I think you will agree with me that such a proceeding would have been rash and unjustifiable.

The progress of the case subsequent to operation was very satisfactory. There was no secondary hæmorrhage, of which I had considerable fear. The scalp wound united by first intention, except at the seat of the drainage tubes, through which some suppurative discharge took place for a fortnight—no doubt owing to the free use of the cautery.

The patient experienced very great relief from his symptoms very soon after the operation, and slept remarkably well. He left the hospital November 24th, feeling wonderfully well, and with the intention of again engaging in business of some kind.

The points which I consider specially worthy of debate in this case are the following :—

1. The probable origin of the tumour.
2. Was it possible to diagnose the precise nature of the

aneurysm previously to operation?—the cirsoid condition of the superficial temporal being suggestive of aneurysm by anastomosis.

3. Was an operation advisable or justifiable?

4. What was the probable intra-cranial condition? I believe it to have been an aneurysm by anastomoses of the meningeal blood-vessels.

5. I may point out that the bones themselves were perfectly healthy, and, therefore, the case was not of the nature of a pulsating bone tumour.

N.B.—Since reading the above paper at the Academy I saw the patient. He said he was perfectly well, and was looking out for occupation. He was perfectly free from head symptoms. Hence, I think I am justified in concluding that there was no intra-cranial aneurysm, and that the head symptoms were due to the dilated condition of the middle meningeal itself.

The President thought that it was not possible to form a diagnosis with any great degree of certainty. He mentioned the case of a girl, aged seventeen, with a pulsating tumour at the junction of middle and lower third of thigh. The tumour had a connection with the bone. There was tremendous hæmorrhage, to check which pressure on the femoral trunk above the tumour was ineffectual. The tumour was supplied from blood-vessels in the bone. He thought amputation at the hip-joint was the right proceeding in such a case.

Dr. Bennett congratulated Prof. Pearson on his paper. He would like to know from Prof. Pearson whether (1) there was any *bruit* heard previous to operation; (2) whether pressure on the carotid was carried out with a view to stopping the bleeding. Pressure on the common carotid could be kept up so completely under anæsthesia that it would control the hæmorrhage, at least for a time. Pressure in Sir. T. Stoker's case would be of no use, but things were different in tumours of this kind occurring in the head.

MR. KENDAL FRANKS asked what was the condition of the bone. From what he could gather there was a fissure in the bone, through which fissure a cirsoid aneurysm existing within the skull was continuous with a similar aneurysm outside it.

MR. THOMSON thought that one of the most interesting features of this most interesting case was its subsequent history, as detailed by Prof. Pearson. He took it, of course, that what was removed was only part of the tumour, for, from what he could gather, he believed there was a tumour within the skull, perhaps not so large, but of the same character as that which appeared outside. Now, when Prof. Pearson removed the outer portion of the tumour, what became of the inside portion if it was of the same character? What happened in Prof. Pearson's case probably was, that the sealing up of the vessels on the outside had the effect of what was called distal ligature. Prof. Pearson's difficulty with the hæmorrhage was in the fact that the tumour was fed by vessels inside the skull. He knew the difficulty of getting ligatures to hold in those cases, but he was not sure that the application of the cautery would be a reliable method in the next case.

PROF. PEARSON, in reply, expressed his thanks to the Academy for the kind manner in which his paper was received. In reply to Prof. Bennett, he said there was no *bruit* present. Compression of the common carotid was resorted to to check the hæmorrhage. He regarded the case not as one of pulsating tumour of bone, but as a case of aneurysm by anastomosis. He regarded the intra-cranial aspect of the case as very serious. He believed, with Mr. Thomson, that there was a free communication between the aneurysm inside and that outside the skull. He mentioned that, in his case, the bones were sound, though much thinned. The arteries coming through the squamous portion of the temporal bone were much dilated.

A REVIEW OF THE CAUSES RECOGNISED AS FACTORS IN TUBERCULAR HIP-JOINT DISEASE, AND ITS TREATMENT BY REST AND FIXATION.

By R. L. SWAN, F.R.C.S.;
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[Read in the Section of Surgery, January 10, 1896.]

It would be impossible, owing to the necessary limitation of time, to consider in detail many of the important points in connection with hip-joint disease. I shall therefore confine myself to two subjects—first, a brief retrospect of the theories held as to the ætiology of the disease; and, secondly, the production of malaise, irrespective of acute local symptoms, in tubercular joint affections, and its subsidence on the enforcement of rest and fixation.

The term "tubercular," as applied to hip and other joint diseases, was until recently a vague though intelligible designation. It was apparently originated by the writings of Nichet, "On Pott's Disease," and of Nélaton, "On Tubercular Diseases of Bone," published more than fifty years ago.

Since that time observation had demonstrated a number of facts which had enhanced the probability of a disease common in joints, being identical with a diathesis which developed local manifestations in dissimilar tissues; that disease of a similar pathognomonic type should attack in succession various joints, such as the knee, hip, spine, or bones of the carpus or tarsus; the incidence or sequence of pulmonary tuberculosis, or tubercular meningitis, during or after the existence of disease in those joints. These

occurrences did not, however, advance our positive knowledge beyond that of Delpech, who, in 1816, asserted the close analogy of Pott's disease with pulmonary phthisis. He stated that tuberculosis was the only cause of that deformity which is called Pott's disease of the spine, and that, therefore, this affection should be termed "tubercular disease of the vertebræ." But it was not till the discovery of the tubercle bacillus by Koch, and its verification by Schuchardt and Krause, Bouilly, Müller, and many other observers, that a number of truths were elicited which have proved that what was so long accepted as a clinical belief had become an undoubted certainty.

Numerous cases have been recorded where wounds in individuals who were previously healthy, having become infected with tuberculosis, bone and joint diseases of the tubercular type have subsequently become developed.

Pfeiffer reports a case of a veterinary surgeon, perfectly healthy, and with a good family history, who, while dissecting a tubercular cow, accidentally wounded his left thumb. The wound healed, but induration of the scar took place. The metacarpo-phalangeal joint became swollen and presented the appearance of a tubercular synovitis. Some months later the signs of pulmonary tuberculosis supervened, and he died of the disease in 18 months after the receipt of the injury. The joint after death showed all the usual destructive changes both in the bones and synovial membrane, and a large number of tubercle bacilli were present.

Czerny records two cases where skin grafting was employed for the treatment of large ulcers, the skin having been taken from limbs recently amputated for tubercular bone disease, and those patients afterwards became the subjects of tuberculosis: one died of phthisis in 14 months, the other developed spinal disease with curvature and

psoas abscess. Cases of a similar nature have been also recorded by Brandon, Peters, Middledorpf, and others.

A youth, aged eighteen, was under my care in Steevens' Hospital suffering from tubercular disease of the neck of the bladder, characterised by the usual symptoms of irritability and discharge of small quantities of sanious pus from the urethra. I sent a specimen of this to the Clinical Research Association, and received in reply—"large number of tubercle bacilli present." He is now, one year after, in an advanced stage of pulmonary tuberculosis.

A proof that those strumous diseases are tubercular is also afforded by a histological examination of the affected tissues. König examined 72 specimens in the Gottingen museum, and of these 67 yielded confirmatory results. He found tubercles in bones and joints, in the walls of chronic abscesses, and in the soft tissues at the reflection of the synovial membrane, and he shows that tubercular tissue is not found in granulations of ordinary inflammation, or in acute osteomyelitis. In addition to the statements already made, that pulmonary tuberculosis is a frequent result of such disease of the larger joints, it is only when such disease is concomitant, or in immediate sequence to the joint affection, that the surgeon will recognise their pathological affinity. Many patients, however, will die at a later period from such supervention.

Cheyne states that, speaking roughly, 20 to 30 per cent. of the patients suffering from strumous diseases of the larger joints ultimately die of internal tuberculosis. König writes that only in 21 per cent. is the joint the sole tubercle trouble, but it is to be supposed that he includes in the larger numbers those who have implication of the neighbouring lymphatics and contiguous peri-articular tissues.

Attention may be directed to the admirable and instructive paper in the *British Medical Journal*, April, 1890, by

Mr. Walter Cheyne, in which he demonstrates the possibility of induction of similar diseases in animals by the injection of tubercular material into bones and joints.

We approach, therefore, the examination of hip-joint disease on the assumption that it is the definite result of the presence of the tubercle bacillus, and that the individual so affected has not alone the local malady to endure, but is menaced with superadded and possibly fatal dangers.

It is interesting to consider at this juncture the probable method of localisation of the parasite. Allusion has already been made to accidental inoculation, and also to the experimental injection of bacilli into the tissues of animals. But in the ordinary cases of strumous joint diseases in children we must look for some other channel.

The following observations embody, I believe, the most recent opinions regarding infection and the conditions which influence it.

There are some cases of primary tuberculosis of joints, kidney, liver, spleen, &c., which appear to be associated with a fœtal hæmatogenous infection (Baumgarten. Gärtner).

But in a large proportion of cases the inoculation is post-fœtal—through the mucous membranes or skin.

The infection of the lymph-glands is usually consecutive to tuberculosis of the skin, but Lesage and Pascal consider that tubercular polyadenitis is often congenital.

Regarding the infiltration of certain groups of lymphatics, it may be looked upon as due to a definite local cause. These groups may practically be named as three in number: 1st. The cervical group. 2nd. The bronchial group. 3rd. The mesenteric group.

The cervical glands are superficial and deep. The superficial lie under the platysma and receive lymphatic vessels from the external ear and side of the head and neck. The deep lie along the vessels and communicate with the tonsils,

mouth, palate, pharynx, and larynx. There are also submaxillary glands which receive tributaries from the lower incisor gums, the tongue, and lower lips.

The bronchial group of glands are very numerous. They may be localised from the names applied to them, as: 1st. Intercostal. 2nd. Sternal. 3rd. Anterior mediastinal. 4th. Cardiac. 5th. Tracheal and bronchial proper.

The mesenteric group are those of the mesentery, those of the lesser omentum and the glands contiguous to the aorta. With this group the pelvic glands are sometimes, though infrequently, involved in tuberculosis.

The appearance and clinical history of tubercular adenitis of the glands of the neck are so familiar to every practitioner that a short reference will suffice. There is reason to believe that they become infected from without by the entrance of bacilli through the mucous surfaces of the throat, especially when diseased through the alveoli, or from extension from tubercular disease. I have seen them consecutive to caries of the cervical vertebræ. With reference to the first method of contamination, the experiments of Cornet upon the presence of bacilli in the dusts of cities and of rooms show how widely the virus is spread, and how likely it is that in crowded centres bacilli are inhaled and even swallowed, and thus that the lymphatics are infected.

It has been suggested that, as the course of this adenitis is slower than the acute forms of disseminated tuberculosis, it is due to an attenuated virus, or it becomes modified in its transmission.

Regarding the implication of the bronchial and mesenteric groups of lymphatics, we believe that the respiratory and digestive tracts admit the bacilli. But in most cases there is no definite lesion at the point of entrance. In the experiments of Klebs, who fed animals on tubercular

material, he found that the presence or absence of tubercular ulcers in the intestine depended on the size of the particles. When presented in large masses, such as cheesy glands or portions of lung, ulcers resulted, but when given in minute subdivision no such lesions were found. Tubercular deposit in the bronchial or mesenteric glands are, therefore, seen in children without any mucous lesions. When we remember that the most probable cause of infection is milk from tubercular cows, an easy solution of the question of the introduction of the bacillus into the lymph channels and thence into the blood is given us. A more difficult problem is to determine the reason of the selection of a special bone or joint for the retention and development of such elements.

In addition to the well-known fact that when non-pathogenic elements are injected into the blood they are rapidly deposited in the medulla of bone, especially near an epiphysis, the developmental changes which are in continual operation in such a position in the young may favour the deposit and growth of the bacillus.

But there is, in addition, that exciting cause, in which every surgeon seems to have an instinctive belief—namely, injury, a history of which he strives to elicit. That this is a potent factor in determining the production of tubercular disease was proved by experiments on animals by Professor Krause, and also by Schüller. It was found that severe injuries, such as dislocations or fractures, were repaired readily and not followed by tubercular disease, but that trivial injuries, such as strains or slight bruises, became tubercular. Cases are on record where fractures occurring in the shafts of bones whose extremities were tuberculous united easily. The preceding assertions are borne out by our usual experience in hearing the history of hip-joint disease, as we find that, although there has been often

some recollection of a fall or bruise, it has quite as frequently been forgotten. It has been supposed that the disturbance of the circulation in the part, similar to that of the early stages of inflammation and necessary to repair, may favour the growth of the bacillus, especially if there has been extravasation of blood laden with these organisms.

We cannot overlook that condition vaguely understood as "receptivity of the tissues." Many influences may contribute to this. While it has not yet been proved that the tubercle bacillus has been directly inherited from the progenitor (up to this, experiment is altogether against it), we all recognise the fact that the delicate offspring of delicate parents will not be likely to lead an outdoor, athletic, or healthful life, but will diminish vitality of tissue by a sedentary method of existence. That the mother may transmit the tubercular diathesis to her offspring is, however, generally admitted. The investigations of Watkins (Pan-American Medical Congress "Transactions," September, 1893) draw attention to certain granular masses in the blood termed third blood corpuscles, which occur in groups or singly, and whose function has been for long a subject of theory or conjecture. These structures are believed to be a pathological element and to declare the presence of a tubercular diathesis. This third blood corpuscle is found in pulmonary consumption; Pott's disease, hip-joint disease, chronic abscess and lupus, and tuberculosis in any form is impossible without its presence in the blood.

The statement has been made by many that certain slowly-acting blood changes, called dyscrasia, precede tuberculosis. Dr. Watkins believes the third blood corpuscle to be the *sine qua non* of the tubercular bacillus, and as such is one of the chief means of diagnosing this dyscrasia which it produces. In a series of experiments he claims to have demonstrated that the tubercle bacillus

will live in acute tubercular blood, but will not live in blood which does not contain the third blood corpuscle.

He states that the third blood corpuscle is very often discovered in the blood of infants born of tubercular mothers, while he has never discovered it in the blood of infants born of non-tubercular women, when from the very beginning they have been nourished by their mother's breast.

The quality of the food has long been recognised as a factor in the production of strumous diseases. Watkins found that a baby in unhygienic circumstances will have the third blood corpuscles produced in its blood in from three to ten days by using sweetened milk and water, or by over-sweet condensed milk.

We know that the carnivora are practically exempt from tuberculosis, while the herbivora are extremely susceptible of its ravages. Man occupies an intermediate position. Bidder, in speaking of the treatment of strumous diseases, lays stress on the avoidance of substances rich in potash, and also of starchy materials, and advises the employment of albuminous foods rich in soda and fat. It is certainly true that an easy assimilation of fat appears to be unfavourable to the development of tuberculosis, and persons with such a diathesis are usually averse from its consumption, and prefer the saccharine and starchy elements of food.

There is a fact which cannot fail to attract the attention of every observer—namely, the development of tubercular disease in joints after debilitating diseases. How often do we see the following: "A healthy child gets whooping-cough, vomits constantly for weeks, becomes emaciated, partly convalesces, gets measles with some chest complaint, begins to go about, and is then found to be lame, and soon exhibits all the symptoms of tubercular morbus coxæ."

It is now generally admitted that in the early stages of

morbus coxæ fixation of the joint is necessary, and most surgeons will adopt traction if muscular rigidity, night cries, or deformity be present. An effort is often made, however, to combine the advantages of open air and gentle exercise—with fixation and traction of the joint—by a traction splint, which combines fixation and traction, or by a Thomas' splint, which confers fixation alone. Other surgeons, amongst the number of which I desire to form one, advocate in early joint tuberculosis, fixation, traction, and recumbency. The non-operative treatment, therefore, in an early stage may be divided into the ambulatory and recumbent.

From the results gained by observation, and the records of a great number of tuberculous diseases of the hip and other joints, which I have collected for the past twenty years, I have arrived at the conclusion that the ambulatory treatment in an early stage is extremely unsatisfactory, and I am satisfied that in discarding it and adopting the system of recumbency and complete rest, I have obtained better results than I should have done had I acted otherwise. There is no one who would more willingly advocate judicious exercise and fresh air if the case were suitable for such concession, but the following reasons have decided my course of action. I do not now confine myself to the hip-joint, but apply the same deductions to all joint affections of a tuberculous character, and select for illustration those of the spine, hip, and knee. I have, in common with all surgeons who have seen many of those cases, observed a condition which is invariable, and to which the vague term dyscrasia has been applied. Take, for example, a child affected with a tuberculous focus in the bodies of two, three, or more dorsal vertebræ. There is no pain, as we know that in children the deposit is, as a rule, intra-osseous. There is a feeling of fatigue. The child exhibits

lassitude, and dislikes play. He looks bad; gets thin, not a healthy leanness, but a fragility. He has dyscrasia. Take a diseased hip with a deposit at or external to the epiphysial line; there is slight flexion and abduction, but no acute pain; no night cries. He has dyscrasia. Take a knee; there are deposits in the bone, slight flexion, which relieves the intra-articular pressure, no pain to speak of, but there is a dyscrasia. This condition, manifestly the outcome of some enervating influence, is seen in children who are not at rest, coming, perhaps, as out-patients to the hospital, or if in-door patients, who are still permitted to go about with a fixation apparatus, such as a Sayer's jacket, an immobilising splint for the knee, or a Thomas's splint for the hip. Now I wish to observe that this constitutional state in patients who are undergoing the so-called ambulatory treatment is invariably accompanied by an elevated afternoon or evening temperature during the continuance of movement; and unless from some special reason, such as the commencement of abscess, the severity of the case is indicated by its rapid progress, and otherwise this temperature subsides on the enforcement of rest. From the records of a large number of cases, all in the above category (that is, in the pre-caseating stage, and without acute symptoms), I select six, in whom the temperatures were recorded while going about, and who were subsequently treated by enforced recumbency:—

		M. Temp.	Ev. Temp.
Shin	boy, 10, involving four dorsal vertebræ - -	92·2°	101°
	boy, 6, two dorsal, two lumbar	100°	102°
Hip	girl, 8 - - -	100°	102°
	boy, 7 - - -	99°	101°
Knee	boy, 12 - - -	99·2°	100°
	boy, 14 - - -	99°	101°

These are illustrations of temperatures which are usually,

I believe, entirely unsuspected and unlooked for in consequence; and accompanying the malaise referred to. Such a temperature as any one of those cases exhibits is not necessarily followed by suppuration. The cases quoted all terminated in recovery without such an event, and the temperature in each instance became normal when rest in bed was enforced. If such a case, at too early a period, be again submitted to the ambulatory treatment, even with every precaution for the local fixation of the diseased joint or joints, an accession of elevated evening temperature is again perceived, and a return of the haggard and worn aspect indicative of the induced dyscrasia. What is the significance of this oft-repeated term? Does it mean that exercise creates a disposition to dissemination of the bacilli? Does it indicate an absorption of toxic material into the blood from the focus of disease? Does it signify that increased rapidity of circulation renders more potential or increases the number of the third blood corpuscle, the so-called *sine qua non* of the tubercle bacillus? We are aware that rapidity of the circulation increases absorption, and that the subject of erysipelas, typhoid, or septic infection will not go about, but remain quiet. We also know that in such cases, so far as clinical evidence can give us information, the increase in temperature, and malaise, do not appear to be due to any alteration at the site of the deposit.

If we contrast the appearance of those patients—their improved digestion, cheerfulness, and vigour—who have been subjected for a time to enforced rest, and the similar conditions of those who have been undergoing ambulatory treatment, we cannot fail to be struck with the contrast, the state of the disease being tolerably similar.

These observations must not be held to imply a disregard of the advantages of fresh air and exercise if the case be

in a suitable condition for such indulgence, but if there be an evening temperature of even 99° during the treatment by a traction splint—or a Thomas's—with patten and crutches combined with daily exercise, I should at once forbid movement and place the patient in bed.

An interesting paper appeared in the *Boston Medical and Surgical Journal*, April 17th, 1890, by Dr. R. W. Lovett, on the diagnostic value of high temperature in joint disease. It has been demonstrated that elevation of temperature is present in connection with the earliest symptoms of hip disease. The presence of an evening temperature of 100°, under those circumstances, greatly increases the probability of tuberculous disease. In the same way obscure affections of the vertebræ, such as slight immobility with a little lateral deviation of the column, may be considered more or less suspicious according to the presence or absence of high evening temperature. The temperature in those early cases must be looked on as a symptom in quite a distinct way from that which accompanies more advanced conditions.

The temperature does not invariably rise during the formation of pus, or in cases of unopened abscess, and when it does so it appears to bear a proportion to the severity of the case rather than to the occurrence of suppuration. Temperature will always exist in the late stage where rapid disintegration is going on and the discharge is also profuse. Also where there are numerous discharging sinuses; it is obviously very difficult to keep such channels aseptic.

A persistent high temperature, with no obvious cause, in the early stages of hip-joint disease, will indicate that the disease will run a rapid and destructive course, and is ominous of an unfavourable termination.

Under recumbency and fixation the temperature becomes,

if not quite normal, at least constant. Any sudden elevation will indicate trouble, either joint implication, or some remote tuberculous deposit. I have seen tuberculous meningitis heralded by a temperature of 103° to 104°, in the progress of hip or spine disease.

From a consideration of the foregoing circumstances, I am forced to the belief that the ambulatory treatment in tuberculous diseases of joints is unsatisfactory, and especially in the case of hip-joint disease, as it is manifestly impossible to so fix the articulation that no possibility of movement can exist, but the general reasons that I have advanced bear even greater weight, as while even a very grave state of malaise may, for a long period, be permitted to exist, the local conditions, if acute, imperiously demand recumbency, fixation and traction.

The President said that if ever there was a paper calculated to elicit discussion it was this. The paper was one of great ability and the result of great practical experience. One of the most interesting matters put forward by Mr. Swan was his dissertation on the existence of tubercle in parts other than where it exhibited itself. That was a question on which they now felt certain. There was another matter on which he entertained a growing opinion, and that was that in the great majority of cases of tubercular spine disease the bones are the centres of the affection. The disease of one joint—the knee—confirmed him in this opinion. Many cases, known clinically as tubercular synovitis, in reality belonged to the osteal class; and abscesses regarded as of the soft parts were old tubercular abscesses, originating in bone.

Prof. C. Y. Pearson supported the statement of the President, that most of the cases regarded as tubercular disease of the synovial membrane have really their origin in the bone.

Mr. Kendal Franks thought that the bone was generally the starting point of tubercular mischief. He had for years given up the operation of arthrectomy. In cases of knee-joint disease, where the ends of the bones looked healthy and the disease seemed limited

to the soft tissues, section of the bone generally showed an abscess in the condyle of the femur or head of the tibia.

Mr. Thomson was very strongly of opinion that in the vast majority of tubercular diseases of joints the cause was due to injury. But in many cases the injury was so slight that it was not complained of until damage had been done to the bone. The larger number of these tubercular joints they saw occurring in children; and they were not accustomed to deal with morbus coxæ after the growing age was passed. He thought it was not a matter of very great importance whether the disease began in the bone or synovial membrane; for, wherever it began, both bone and synovial membrane were engaged at the time they saw the disease. Personally, he believed the disease began in the bone and spread to the synovial membrane. He was strongly opposed to erasion. He never did an erasion, and so far as he knew he would never perform the operation, because it was theoretically bad, and its results were such as did not encourage one to give it a trial.

Mr. Chance congratulated Mr. Swan on his paper. The only point on which he differed from him was in regard to his treatment by adduction. He thought Mr. Swan's method of extension an excellent one, but inferior to extension by a wire splint he described.

The President said he had been looking for a case suitable for erasion, and had not found one. An erasion had been done in a case elsewhere, seven years ago, and supposed to be cured. The operation of excision had become necessary and showed that the bones were full of tubercular centres.

Mr. Swan thanked the meeting for the manner in which they had received his paper. The speakers, he said, were unanimous in their condemnation of arthrectomy. He, too, condemned it.

A NEW METHOD FOR LOCALISING BRAIN LESIONS.

By ROBERT H. COX, F.R.C.S.I.; Wu-hu, China.

[Read in the Section of Surgery, February 21.]

THAT there is need for a more perfect method of locating the important areas of the cerebral cortex on the overlying surface of the scalp will, I think, be admitted by you all, seeing that most, if not all, of the methods generally employed are either limited in their scope or complicated in their application, while many give rise to error by the use of a standard measure—the inch or the centimetre—for varying surfaces.

The method which I am about to describe will, I think, be found devoid of these objections. In the use of this method two things are necessary, viz.:—

1. An instrument which I have invented and called (for want of a better name) a cerebro-graphometer; and—

2. A diagrammatic map of a hemisphere of the brain, prepared from readings made by the use of the same instrument on the cadaver and casts of the brain *in situ*. This map may, however, be replaced or augmented by a list of indices made in the same way.

The instrument consists entirely of the mechanical device known technically as "lazy-tongs," formed into a circle with two accessory loops. The circle, or base, contains 28 fenestræ, and has attached to it externally, at four equi-distant parts, the two accessory loops by their end rivets (excepting one, where the attachment is at the rivet third from the end) in such a way that the loops arch over the circle at right angles to each other, while at the same time they are capable of a

hinge-like movement. One loop bears, on its central rivets, the numerals, beginning at each end with 1 and ending in the middle with 10; while the other loop has the letters A to V in consecutive order from before backwards, the rivet marked T being that connecting the loop with the circle at this end.

The map is made on a gnomonic projection, with the radii or longitudinal lines marked by letters and the semicircles or lines of latitude by numerals. The tracings in this particular case were taken from one of Professor Cunningham's beautiful casts. (Fig. 12, Cunningham Memoir, published by the Royal Irish Academy.) The list of indices here shown is also from the same source.

To use the instrument it is necessary to extend it in all parts and apply it to the human head, with the rivet forming the junction of the lettered loop with the circle (below that marked A) on the glabella, and the rivet marked V (at the extreme end of the same loop) to the occipital protuberance, when the loop between these two points should be pressed down to the scalp in the middle line, while the circle is also closed on the skull on such a plane that the central rivet, marked 10, of the numbered loop shall lie on the middle line as shown by the lettered loop. The instrument is thus fixed to the skull by the circular band and the lettered loop. Whilst the numbered loop is capable of movement backwards and forwards with a hinge-like motion, during which each numbered rivet marks a semicircle of latitude. The portion of the same loop, from one of the fixed rivets to the numeral 10, will, as it extends to each letter, give the radii of longitude. With the instrument thus fixed in position, to find any given point—say, Broca's lobe—it is only necessary to consult the map (or the list of indices) for the bearings—there given as "$\frac{C}{4}$ left side"—when, by moving the 10 on the numbered loop forward to C and pressing the loop

Fig. 1.

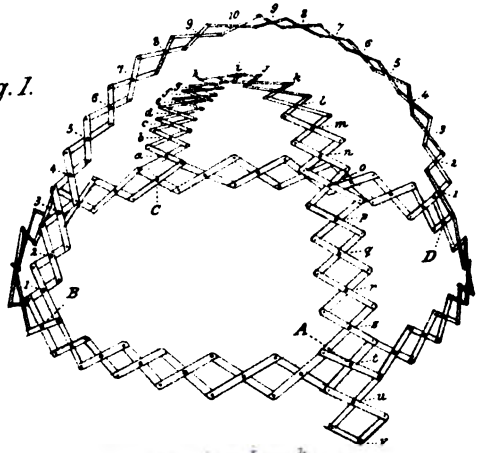
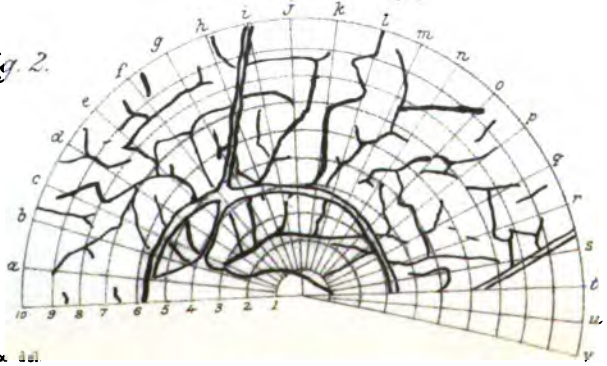


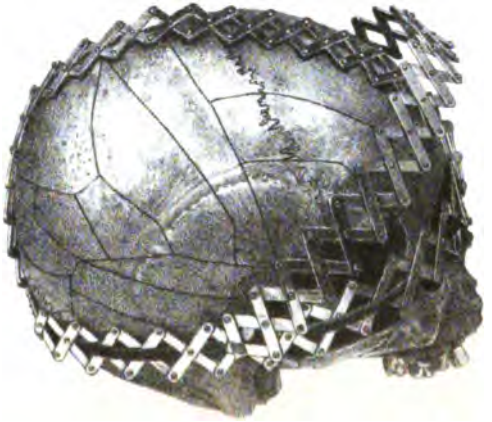
Fig. 2.



R. H. Cox. del.

Fig. 3.

Fig. 4.



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List of Indices to important Regions of the Brain Cortex.

Description	Index
Motor Centres : External Aspect :—	
Inferior extremity	$\frac{L\ K}{8}$
Superior	$\frac{K}{6}$
Face	$\frac{I}{4}$
Lips and Tongue (Broca's lobe)	$\frac{C}{4}$ (left side)
Head and Eyes	$\frac{I}{7}$
,, *Mesial Aspect :—*	
Leg	$\frac{K\ L}{10}$
Trunk	$\frac{J\ K}{10}$
Arm	$\frac{I\ J}{10}$
Head	$\frac{H\ I}{10}$
Sensory Centres :—Hearing	$\frac{J}{1\frac{1}{2}}$
Sight	$\frac{R}{6}$
Special Convolutions : Frontal :—Ascending	$\frac{I}{4}$ @ $\frac{K}{9}$
Superior	$\frac{C\ @\ J}{8\frac{1}{2}}$
Middle	$\frac{C\ @\ I}{6\frac{1}{2}}$
Inferior	$\frac{E}{4}$
,, *Parietal* :—Ascending	$\frac{J}{3}$ @ $\frac{L}{8}$
Superior	$\frac{M\ @\ P}{9}$ @ $\frac{N}{7}$
Supramarginal	$\frac{M\ N}{4\frac{1}{2}}$
Angular	$\frac{O}{5\frac{1}{2}}$
,, *Occipital* :—1st	$\frac{Q}{8}$
2nd	$\frac{S}{8}$
3rd	$\frac{U}{8}$

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List of Indices to important Regions of the Brain Cortex—con.

Description		Index
Special Convolutions: Temporal:—1st		$\frac{C}{2} @ \frac{N}{3}$
2nd		$\frac{J}{1} @ \frac{Q}{4}$
3rd		$\frac{T}{3}$
Special Fissures:—Rolando's		$\frac{H}{3\frac{1}{2}} \frac{I}{} @ \frac{L}{10}$
Sylvius'		$\frac{E}{2\frac{1}{2}} @ \frac{M}{3}$
Middle Meningeal Artery		$\frac{E}{3}$
Cerebellum		$\frac{V}{7}$

NOTE.—The Letters and Numerals correspond to those on the instrument. When two letters are placed thus—L K—it means a point half way between them. When an index is given thus—$\frac{I}{4} @ \frac{K}{9}$—it means the portions at these two points and that between them; when like this—$\frac{M @ P}{9} @ \frac{N}{7}$—it means that portion from M to P on the line of latitude marked 9, down to N on the line marked 7.

EXPLANATION OF PLATE.

FIG. 1.—The instrument extended.

A B C D=circle, or base.
C A F=longitudinal loop.
B D=latitudinal loop.
F=point applied to occipital protuberance.
C=point applied to gabella.

FIG. 2.—A map (on a gnomonic projection) of the surface of the right hemisphere of the brain, made, with the aid of the above instrument, from a cast of the brain *in situ*. It shows the principal fissures and convolutions; bars of bone corresponding to the coronal, lambdoid and squamous sutures, and the temporal ridge.

FIG. 3.—Diagrammatic map (on a gnomonic projection) of the external surface of a hemisphere of the brain, showing the boundaries of the lobes and convolutions.

FIG. 4.—The same projected on the right side of the skull. Also shows the instrument in position for localising.

down on the scalp, the numeral 4 will lie over the part. Rolando's fissure may be found in a similar way by locating each end and joining the two points by a straight line.

Localisation by this method may be summed up under two heads—(*a*) Application of the apparatus, and (*b*) Location of part sought for.

(*a*) *Application of the apparatus.*—Extend the instrument and apply the end of the lettered loop marked V to the occipital protuberance and the other end to the glabella, then press down the loop to the scalp in the middle line, and close the circle round the head so that the 10 on the numbered loop will lie on the lettered loop.

(*b*) *Location of part sought for.*—Consult either the map or list of indices for the bearings, place the numeral 10 on the letter of longitude and press the numbered loop down, when the numeral of latitude will rest over the part sought for.

Having thus shown the simplicity of the method, for the carrying out of which no recourse need be had to the science of mathematics in any of its branches, and the almost automatic action of the cerebro-graphometer, I would now ask your attention while I try to explain its accuracy.

As may be seen at a glance, the instrument is equally applicable to all sized heads, and forms its own unit of measurement for each, thus eliminating the errors due to the use of a standard measure for a varying surface.

The two fixed points which are taken are easily found, and are those which have proved most reliable—the glabella and the occipital protuberance—and these place the circular band, or base upon which the accessory loops hinge, on that plane of which Professor Fraser has written in his "Guide to Operations on the Brain"—"and, finally, the key-note of all the remarks which are to follow is this:—That for normal heads after birth the position of any structure of importance

in their cavities varies proportionally with the variations of the circumference as given by the circumferential tapes of the respective heads." If this be true, or even approximately so, it points to this plane as the most likely one to afford the minimum amount of variation in the position of the cranial contents. Of course the markings of no two brains are the same, therefore no mechanical method can give perfect accuracy, but I hold that my method is the nearest approach yet to that goal, which, indeed, seemed to be reached with reference to Rolando's fissure in the only cases I had an opportunity of testing it.

That some of the bearings given may not prove to be the average, when tested by others, I am prepared to find, seeing that the material at my disposal has been very limited, but the investigation of those whose opportunities are greater would soon remedy this.

In conclusion, I would add that the cerebro-graphometer is simple, compact, durable, and capable of being rendered aseptic by boiling.

Messrs. Arnold & Sons are the manufacturers.

The President said he had examined this ingenious instrument already; it would doubtless prove of value to surgeons in mapping out the regions of the brain for operative purposes.

Prof. Cunningham said that, from the point of view of the anatomist, the instrument had an interest for him. He contrasted it with an instrument invented by Prof. Areyd, of Moscow, to the advantage of the former. He was glad that a gentleman who had studied in Dublin had invented such an instrument.

Prof. Fraser expressed a high opinion of the instrument.

Dr. Heuston had tested the instrument by the results of several experiments. Operating surgeons would come to look on this instrument as one of practical value.

Mr. Cox, in reply, said the instrument was yet in an incomplete state.

CANCER OF THE BREAST.

By WILLIAM THOMSON,
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Surgeon to the Richmond Hospital, Dublin, &c.

[Read in the Section of Surgery, March 20, 1896.]

We have become so much accustomed to discussions on the advanced surgery of to-day that I feel almost bound to apologise for asking your attention for a short time to the apparently common-place subject of cancer of the breast. It is a little difficult to avoid the fascinations of enterectomy, or some of the other operations upon the abdomen or its contents; or those even bolder procedures in brain exploration which are sometimes so successful and satisfying to the operator. But these are not of the ordinary every-day work of the surgeon. On the other hand, the breast and its tumours are the subject of constant experience either for diagnosis or operation. They are the cause of great mental suffering; there is hardly a woman who does not know that troubles in that organ are common, and very many who, at some time or other, believe that they have a "lump," or live in dread that they may get one. Now, a field of surgery like this, although it may be ordinary, is a very important one, especially when we have to deal with any form of malignant disease, with all its known persistence and resistance to treatment. That being so, I think the surgeon may pause and ask himself whether, in the general advance of operative work, this most common and terrible plague of cancer of the breast has received the close attention that it deserves; whether it is one of the few things which have baffled his enterprise, and remain pretty much where they were.

I remember that, some years ago, the late Mr. Rawdon MacNamara read a paper on this subject before the Surgical Society of Ireland, and that he gave the gloomiest view of the results of operation. He was speaking as a professed unbeliever in the principles or powers of antiseptic and aseptic surgery, and of the promise which it held out to us as to safety of severer methods of operation. But at the time he was right in his contention, that operation was practically a hopeless proceeding, and that at the best—which might be doubted—there was only the prospect of a slightly prolonged life, and the chance that a recurrence would not take place locally.

Well, we have advanced somewhat upon that view of the situation. If it were true, I think we should find very few cases upon which operation would be at all justifiable, and we should be forced to leave the unhappy patient to the poor solace of anodynes to smooth her way to the grave. Not only that, but we should perforce abandon all operations for cancerous disease of whatever form. The fact remains, however, that we have not done so; nor is there the least likelihood of such an attitude being taken. We operate upon cancer of the lip, of the rectum, of the tongue; and we do so, not with the purpose of giving a temporary relief only, but with the distinct impression in our mind that the disease may not recur at all, and that the patient may be cured.

Why, then, should we shut out from ourselves the view that cancer of the breast may be cured? I believe that it can be; I have, myself, had two patients in whom there was no sign of any return six and seven years after operation. They were both cases of undoubted scirrhus, verified by microscopic examination. One I saw six years ago, and she may be alive yet. The second I have seen within the past three months. She is absolutely without a sign or symptom

of return of the disease. Two cases are at present living, without any return, and have almost reached Volkmann's limit of safety—three years. Other surgeons can relate the same satisfactory results. Mr. Mitchell Banks and Mr. Watson Cheyne report 21 and 19 per cent. of cures respectively; and the latter now claims that he has had as much as 57 per cent. under improved methods. Even those who hold strongly that cure is exceedingly problematical admit that it does take place in ten per cent. of the cases, and this admission is enough for my purpose, when I contend that the position of amputation of the breast for cancer is to-day more satisfactory than it has ever been, and that with thoroughness and care a considerable number of the cases which come before us may be saved.

It used to be the fashion with surgeons to rest content with removal of the breast alone; or to abandon the case altogether if the glands in the axilla were engaged. I remember that when I was beginning to practise, a wise consultant dissuaded me from operating in a case for this cause alone. I saw no reason why the glands should not be removed as well, but the patient adopted his view. Herein lies the first important advance—the thorough removal of the affected glands from the axilla. In my experience there is nothing so likely to deceive one as the condition of this space before operation. It is very deep, and sometimes there is no indication that the disease has yet laid hold upon the glands. But a free opening out of it will expose small clusters of the glands, quite sufficiently engaged to perpetuate the disease. They must be all cleared out, with careful patience, until the surgeon is satisfied that only vessels and nerves and undiseased tissues remain behind.

But some surgeons do not rest content with this. Mr. Cheyne, for instance, makes very extensive incisions and removals. He holds that the breast is a much wider spread

organ than we have imagined, and that it reaches as far as the sternum, the clavicle, and the origins of the abdominal muscles. But the important point is, that the whole of the lymphatics, as contained in the fascia, over the great pectoral, and the chain which passes along its edge into the axilla should be completely removed; for in these lymph vessels there is lurking disease, even if the glands have not been apparently engaged. I have practised this method for some years, as have my colleagues, and with some encouraging results. When I have found adhesions to the muscle, portions of this have been cut away; and in some cases we have divided the great pectoral freely, in order to follow up any glandular disease which may have extended towards the clavicle.

But we have got a step further. Most surgeons have, I think, regarded the enlargement of the supra-clavicular glands in the posterior inferior triangle of the neck as a well-defined contra-indication.

In a certain number of cases it is no longer so. The space is now opened above, and the whole chain of the lymphatics is pursued up under the clavicle from the axilla, and cleared away. This may sometimes be done quite effectively, but with less chance of ultimate recovery. Where this condition exists, the first glandular line of defence has been overcome, and we have in most cases, I fear, reached the limits of interference. Still the operation is not very formidable, and any additional risk is worth the chances of improvement. Of the further advance—amputation at the shoulder joint, and division of the clavicle—I have no experience, but it seems to me to introduce an enormous risk without any compensating advantages.

I pass from this to refer to another question upon which very much depends—Is cancer produced locally, or is it the result of an already existing constitutional state? I believe that in

the great bulk of cases it is at first purely local, although I know that there are many who regard it as the local expression of a diathesis. So far as clinical experience goes, I hold that we have no evidence to show that the patient is already cancerous when the first signs of the new growth appear. If I were convinced of the truth of that contention I should not operate for cancer at all, because I should not have the dimmest hope that I could do more than palliate. I admit that we find this disease occasionally developing in successive generations from an original stock. That is not a mere coincidence. I have no doubt that in some of these cases there is inherited a peculiar constitutional state which may render the individual more liable to the results of prolonged irritation applied to some particular tissue. But admitting all this, we cannot get beyond a suspicion that in a particular case the hope of ultimate recovery may be less than in one in which we have no history of possible taint. We know nothing of the existing condition, whether the bias that may be given towards the development of the disease is a strong or a weak one. We *judge* that it is certainly not a state of saturation; we *know* that there is no apparent pathological change. There is no peculiar modification of type by which we can recognise it as we do inherited syphilis or tuberculosis. If there is a taint at all it appears to be passive, not virulent; it has not the power to set its impress upon the whole organism, as does either of the diseases I have just mentioned. Compared then with tuberculosis, which is admittedly constitutional, cancer appears to be less potent in effecting those changes upon the individual, which make it easy for us to identify them. There is unquestionably in these cases a peculiarity of contour and of structure, to which the so-called cancerous diathesis has no correspondence whatever. There is no character that we know of that will indicate to us an existing cancerous tendency.

May we not then draw the conclusion that tuberculosis in heredity is able to influence the body more profoundly than cancer?

I think that is a fair conclusion; but let us see where it leads us. The true believer in a cancerous diathesis has no faith in the possibility of cure by the removal of the tumour. Ask him why and he will tell you that the tissues are already poisoned by the disease—not, remember, as the result of infection from the local outbreak, but by an inherited virus. He can point to nothing more tangible than a family history; there is no physical alteration of any kind—apart from the tumour—upon which he can lay his finger and say that that indicates an undoubted cancerous diathesis. Now, observe how illogical he may be. We bring him to a case which, so far as the admitted appearances go, is stamped everywhere with the signs of inherited tuberculosis. A joint has become diseased, but he does not despair; he will excise it or amputate a limb—not to palliate but to cure, and to cure by removing the local affection, although he leaves behind it the inherited tendency which is marked on every feature. I confess I do not see how these two views can be reconciled.

I have no intention to minimise the gravity of cancer. It is to me the most terrible and unsatisfactory disease with which we have to deal as surgeons; but, thinking as I do, that in its first stages it has only a local virulence, I wish to say so much in encouragement of operation. There is a period during which its progress is slow, depending sometimes upon the degree of natural vascularity, as in the tongue; sometimes upon the acquired increased vascularity which follows the birth of a new growth. The process of general infection is delayed by the lymphatic glands, which I call the first line of defence. The activity of the poison is at first centred in the initial growth, and if we see this early I believe we may remove it with a reasonable hope of success.

But no matter whether we are called late or early, the important law must always be obeyed—to clear out the axilla with the greatest care.

I should like very much to deal with another important aspect of this subject—the diagnosis of tumours sometimes mistaken for scirrhus. But it would occupy too much time now, and I must postpone it for another occasion.

I have avoided bringing before you long and quite uninteresting details of numerous cases. I have rather tried in a short paper to raise a few points for discussion; to awaken more interest in a disease and its treatment which we have perhaps looked upon with too little hope, and to show that the outlook for it is less dark than it has been. It is beyond doubt that breast scirrhus is, under certain conditions, a curable affection in a fair percentage of cases. At present that percentage is not as high as I think it will become under closer attention to operative methods. We may look, too, for its earlier recognition; but given this, with its co-existing favourable conditions of mobility and slight infection of the axillary glands, I think we may expect still better results than we have yet attained.

The President said, until he had commenced to operate he had not accurate ideas as to the areas covered by the breast—an area more extensive than anatomists commonly supposed. It extended inwards as far as the edge of the sternum, and upwards as high as the clavicle. It was amongst the poorer classes that desperate diseases of the breast were usually met with. Personally he took a very optimistic view of properly performed operations for cancer of the breast; but of nothing was he more sure than that if some of those operations were properly performed, others were shockingly botched. Some people made too light of operations on the breast, and the results arising from their temerity in this respect were truly shocking. Removal of the breast was of itself a comparatively easy operation; it might be undertaken by anyone with a

moderate amount of skill and experience; but dissecting out the axilla, or lower triangle of the neck, was a critical operation, and one that was very frequently incompletely performed. Operation on the mammary gland for cancer held out a prospect of radical cure. In the matter of sutures he expressed a predilection in favour of silk sutures.

Mr. Nixon said that every surgeon of experience would agree with the views expressed in Mr. Thomson's paper. The necessity for early operation in every suspicious case should be insisted on. An essential element in the operation was that of keeping wide of the disease. He gave the history of a case he had operated on. For the seven and a half years during which she lived after the operation there was no manifestation of a recurrence. A microscopic examination of the breast put it beyond doubt that the disease was scirrhus.

Prof. Bennett said there was one point that required careful attention—that was, the necessity of endeavouring to discriminate between the cancers of the breast. Mr. Thomson had not dealt with the subject of diagnosis. He mentioned the case of a woman who suffered for twenty-three years from atrophic cancer, and ultimately died of sheer old age. It was an important question for the surgeon whether or not he was dealing with a case of atrophic cancer. He would offer a word of caution against operating in a case of atrophic cancer, as the patient will live longer if the disease is not subjected to operative interference.

Mr. Lentaigne was of opinion that on the question of operation for atrophic cancer Prof. Bennett erred on the side of over-caution. When cancer was left alone the result at best was a foul ulcer—a consideration, he thought, in favour of operation. He agreed with Mr. Thomson as to the undoubted and increasing immunity enjoyed by some patients after operation. Amongst others, he could produce one patient who was operated on for a rapidly-growing cancer seven and a half years ago. There was no recrudescence of the disease, and she was now well. She had had two children, both of whom were suckled at the remaining breast. A microscopic examination testified to the cancerous nature of the growth. He dwelt on the influence of locality as a cause of cancer. He instanced three cases of cancer that came from the same house, the inmates of which altogether numbered only eight. He believed this house and its environments were infected with cancerous virus. He now usually performed

Halstead's operation, and removed the pectoralis muscles in severe cases. Contrary to what might be expected, removal of these muscles was not followed by much loss of power of the arm. Indeed, he thought there was often greater power and mobility than when those muscles were not removed in the operation. In the latter case the formation of adhesions often rendered the muscles useless.

Mr. Franks said the risk to which the patient was exposed by letting the disease take its own course was greater than that incurred by operation. He took a favourable view of operations. The good results of operations nowadays were due to the fact that these operations were more extensive than those performed formerly. He pointed out the necessity of keeping wide of the disease and clearing out the axillary glands. He spoke favourably of Styles' nitric acid method of determining gland tissue. Binding the arm to the side to produce absolute rest was, in his opinion, an absolute mistake.

Dr. McWeeney had tried Styles' method at Mr. Lentaigne's suggestion, and he spoke favourably of it.

Dr. Alfred Smith said one of the cases mentioned by Mr. Lentaigne as cured of cancer of the breast for over six years, he knew very well. She was a patient of his; he attended her at all her confinements; she nursed her children on the right breast. There was no redness or congestion in the cicatrix of the removed breast. He congratulated Mr. Lentaigne on his fine result.

Dr. Doyle protested against the indiscriminate way in which cancers of the breast were spoken of. He agreed with much that Prof. Bennett had said. He insisted on the necessity of taking every precaution in order to diagnosticate between an atrophic and an ordinary progressive carcinoma. Instead of operating hastily, four to six weeks should be allowed to elapse before coming to a conclusion. Many tumours of the breast, put in the category of cancer, were not cancerous in their nature at all. He supported this statement by the history of two cases that came under his care. These ladies were occasioned great anguish by reason of having been told that they had cancer. He, Dr. Doyle, satisfied himself that the disease in each case was simple enlargement of the gland. Many of the so-called cures of cancer were not cures at all.

Mr. T. E. Gordon said, on the inner side of the breast a chain of lymphatics accompanies the internal mammary artery. Did Mr. Thomson find those glands enlarged *post mortem*.

Mr. Thomson, in reply, said he was glad that his paper was the means of eliciting an opinion which he already knew was held by Dublin surgeons—namely, that operation for cancer of the breast is a proper proceeding. It should, however, be recollected that there was a section of the profession which did not assent to this opinion; and, indeed, that in itself was a reason why he was happy to have afforded the Surgical Section of the Academy an opportunity of formally expressing its views. Professor Bennett observed that he (Mr. Thomson) had not dealt with the question of diagnosis. That was true; but that question was too large and complex to be dealt with in a paper like that which he had read. In reference to atrophic cancer, perhaps one *would* not be strongly inclined to operate if he was quite sure of it. But given a lump in the breast, Professor Bennett or Mr. Doyle *had* said nothing which would enable the surgeon to determine whether it was an atrophic cancer or not. He dwelt on Mr. Doyle's view of waiting four to six weeks, and pointed out that with the growth of the tumour there occurs increasing vascularity, and therefore increasing danger of infection; so that the sooner the surgeon acted the better for the patient. Even experienced surgeons saw only few cases of atrophic cancer. Looking back over a period of twenty years that he had been an hospital surgeon, he could count the number of cases of atrophic cancer he had met with on the fingers of one hand. To him (Mr. Thomson) Mr. Doyle's argument seemed to be that, because there was this possible thing called atrophic cancer, therefore no operation in cases of breast tumours was justifiable. For his own part, he would rather think he had operated on twenty atrophic cancers than that he had left five scirrhous cancers untouched. In reply to Mr. Gordon, he said his (Mr. Thomson's) experience of these cases *post mortem* was not extensive, but his impression was that the infection of the lymphatics passing into the thorax depended very much on the position of the tumour itself. The deeper it was the more likely the thoracic lympathics were to become infected. On the other hand, the more superficial and external the origin of the tumour, the greater the probability was that infection would extend into the axilla. He thought Mr. Gordon's point deserved the attention of surgeons.

OBSERVATIONS ON THE RADICAL CURE OF INGUINAL HERNIA.

By FRANCIS T. HEUSTON, M.D., F.R.C.S.;
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Consulting Surgeon to the Coombe Hospital.

[Read in the Surgical Section, March 20, 1896].

DURING the past few years so many methods have been introduced for the radical cure of inguinal hernia, that the present seems to me a proper time to bring the subject forward for discussion, with the hope of coming to a definite opinion as to which method is likely to give the most satisfactory results to the surgeon and the patient. It is so well understood that this affection is due to an abnormal condition of the internal abdominal ring, and of the structures entering into the formation of the posterior wall of the inguinal canal, that I would wish to say a few words as to the anatomy of this region. As to the internal abdominal ring, it is most important to remember that it is rendered the weakest position by the coalescence of the structures forming the cord, and the prolongation on those structures of the fascia transversalis as the infundibuliform fascia. The posterior wall of the inguinal canal is naturally divided by the deep epigastric artery and its venæ comites, by far the most important in relation to hernia being that portion external to the vessels which is formed by the peritoneum, sub-peritoneal fascia, fascia transversalis, the reflected fibres from the internal oblique and transversalis muscles to Poupart's ligament, and deep crural arch, "Cooper's fibres," with occasionally some fibres of the transversalis muscle close to the ring, "Guthrie's fibres;" this, constituting the weakest

portion of the posterior wall of the canal, is usually implicated in oblique hernia, and liable to be the seat of recurrence after operation. Of that portion of the canal internal to the epigastric vessels we need consider only that situated between those vessels and the outer border of the rectus muscle, corresponding to Hesselbach's triangle, and the usual position of direct herniæ. Here we find the following structures :— Peritoneum, sub-peritoneal fascia, fascia transversalis, conjoined tendon of the internal oblique and transversalis muscles, the inner and strongest portion of Cooper's fibres, and the outer fibres of Colles's triangular ligament, which render the canal in this position much stronger than that portion external to the vessels. Of the structures forming the posterior wall of the canal, by far the most important is the fascia transversalis, as can readily be demonstrated on dissecting this region, when, if the structures forming the abdominal wall superficial to it be removed, there will be no tendency to protrusion, even if the internal pressure be great; but when the fascia is removed, slight pressure is sufficient to cause protrusion. When a hernia occurs there is not only a weakening of this fascia at the point of exit, but from the result of its pressure there is a general weakness or sagging of the fascia in the inguinal region. I, therefore, consider that in any operation aiming at radical cure, it is not alone necessary to close the opening or prolongation of the fascia, where the hernia has found its way out, but also to strengthen and support the weakened fascia in the immediate neighbourhood, it being my belief that the frequency of recurrence noticed after some of the recent operations is due in a great extent to neglect of this point. So many operations have been recently advocated, that it would serve no good purpose to mention them in detail. I will, however, shortly describe a few of those in most general favour :—

Bassini.—Opens anterior wall of canal to full extent, separates sac from cord, ligatures neck of sac, removes sac if it be small and hernia incomplete, but allows it to remain if it be large; sutures conjoint tendon to Poupart's ligament beneath the cord, anterior wall of canal united, cord remaining in canal.

Kocher.—Separates sac from cord, passes sac through opening in aponeurosis of external oblique corresponding to exit from abdominal cavity, twists sac,* and fastens it to external oblique in direction of inguinal canal.

Halsted.—External oblique, internal oblique, and transversalis muscles cut from external ring two centimetres above internal ring, sac separated from cord and removed, veins of cord excised, fascia transversalis and muscles sutured, cord removed from canal and fixed to superficial fascia of inguinal region.

Mitchell Banks.—Sac separated from cord, neck ligatured as high as possible, sac removed, canal sutured by silver wire.

Ball.—Sac separated from cord, neck of sac twisted and ligatured, sac removed and canal sutured.

MacEwen.—Sac separated, puckered, and retained in canal by suture, canal sutured.

It will be seen that those operations can be divided into—1st. Where the inguinal canal is freely opened; 2nd. Where the inguinal canal is not opened. Taking statistics, we find that the tendency to recurrence after the first is much less than after the second, but I question if the results will be so favourable after a lapse of years, as time is such a potent factor in causing the absorption of cicatricial tissue. There can, however, be no doubt that operations which give a full and clear view of the floor of the inguinal canal, enable the operator to see which of the structures in a given case require most attention.

* Does not twist sac in recent operations.

I will now describe an operation which I have performed on thirty-two occasions, the patients varying in age between eighteen months and fifty-three years, hospital and private. All of the patients recovered, and as far as I have been able to ascertain there has been no recurrence of the hernia, although the cases were not in any way selected ones, some being of many years standing and of a very severe type.

The skin in the inguinal region is drawn downwards, and an incision of about three inches in length is made over the inguinal canal, the inner extremity of the incision corresponding to the crest of the pubes; the aponeurosis of the external oblique and the external abdominal ring are thus exposed. The inter-columnar fascia being opened, the sac is found and separated from the cord. Should the hernia prove to be a congenital one the sac is cut across above the testicle, and the lower portion sutured to form a tunica vaginalis. The sac is now separated from the cord in its entire extent; it is thus saved from injury in the subsequent steps of the operation. The sac is twisted sufficiently to render its neck a solid cord, which is transfixed at its exit from the external abdominal ring by a needle armed with strong gut, which is then tied by a Staffordshire knot. A Wood's hernia needle is then passed through the aponeurosis of the external oblique, close above Poupart's ligament, and at a point corresponding to the exit of the twisted sac from the abdominal cavity; the surgeon's finger in the inguinal canal determines the proper place, also the depth to which the needle should be passed, and protects the cord; the needle is now passed through the twisted neck of the sac, which should be drawn down as well as possible, as the higher the sac is pierced the better. If the curved needle be now lateralised, its point, guided by the finger in the canal, can readily be protruded through the external ring and armed with a strong catgut suture; the needle is now withdrawn. The surgeon's

finger is again passed into the canal, and the needle is again passed through the abdominal wall, about half an inch above, and external to, the exit of the twisted neck of the sac from the abdominal cavity; it should be passed sufficiently deep to include the internal oblique and transversalis muscles with the transversalis fascia, and having reached the canal is guided by the finger through the external ring. It is now armed by that end of the suture protruding from the ring and withdrawn. A second suture is passed in a similar manner through the walls of the canal about midway between the former and the external ring, it again piercing the twisted sac and deep abdominal muscles and fascia on a lower plane than the former. The ends of the ligature which was originally applied round the sac are now passed respectively through the superior and inferior pillars of the external abdominal ring. The deepest suture is now tied, drawing the sac upwards and outwards, and at the same time approximating the deep abdominal muscles and fascia to Poupart's ligament and twisted sac; the second suture is now tied approximating the conjoint tendon, fascia transversalis, and sac to Poupart's ligament, in the middle third of the canal, the effect being that the posterior wall of the canal is closed throughout, by the approximation of the superior and inferior walls, assisted by the twisted neck of the sac, which thus hinders any tendency to protrusion. It will be seen that both the sutures pass superficial to the cord, which is retained in a position somewhat below that which it normally occupies. The operation is now completed by removing the sac beyond the third ligature, which on being tied closes the upper part of the external ring and fixes the cut end of the sac against its internal aspect. A drainage tube is inserted into the canal, the superficial fascia united by a buried suture of catgut, and the skin by silk-worm sutures. I usually remove the drainage tube on the third day, the silk-worm

sutures about the fifth or sixth, and allow the patient out of bed in about three weeks, a spica bandage being applied for another week.

The PRESIDENT said that the great elaboration of detail in operations of this kind showed that none of them contained the principle of success. He himself had operated on a large number of ruptures, and regarded the operation as free from danger when carefully performed. He had long been of opinion that the one thing necessary to ensure success was not any particular method, but the performance of the operation in such a way that, while temporary closure of the inguinal canal during healing is afforded, a copious lymph exudation is produced. He regarded the sutures as serving two purposes—one the temporary closure of the part during convalescence, and the other the provocation of a copious lymph exudation, which is the real means of producing contraction and permanently drawing the parts together. He believed that congenital phimosis was a very frequent cause of inguinal rupture in children. In congenital inguinal rupture he drained the scrotum.

MR. CROLY had tried nearly all the methods mentioned, but within the last fortnight he had performed two operations by the displacement method of Kocher. Mr. Heuston had quoted Kocher as twisting the sac, but Kocher had now given up this practice. If the sac was twisted freely, besides causing necrosis, a bit of the gut was very apt to be nipped. He avoided scrotal wounds when possible, as asepsis was very difficult there. He was surprised to hear catgut recommended for giving support to parts—a stout piece of sterilised silk was better.

MR. BENNETT said that in the present day there was a tendency to the indiscriminate adoption of cutting operations for the radical cure of hernia. In children with congenital inguinal hernia, with proper attention the use of a truss will, in 70 per cent. of the cases, bring about a cure. In operating he followed Mr. Ball's method, which was very successful. He had seen no bad results follow from twisting the sac. Twisting was done sufficiently but not to strangle.

MR. FITZGIBBON, since he first adopted Ball's method, had never performed a radical cure without trying to use it. Twisting the sac was most dangerous if it was not isolated from everything else. If the sac could be perfectly isolated, he considered twisting as the ideal method of closing it.

Mr. M'Ardle said that the shortening of the transversalis fascia and the cutting away of the sac entirely was the proper operation. If one tried to close the internal abdominal ring it should be done by transversalis fascia. The peritoneum has no influence in preventing a hernia. In inguinal hernia, if the sac is twisted and left it acts as a guide for a hernia to come down. Since 1882 Kocher had not advocated twisting the sac. The great danger in twisting the sac is that a piece of the intestine might be twisted. In one case he had seen, the greater part of the posterior wall of the sac was formed by the cæcum. He agreed with Mr. Croly that catgut was of no use as a permanent support. He thought that œdema of the scrotum was due to tearing the sac too far down and causing traumatic mischief. He did not resect more than an inch of the sac. He never drained unless there was a great deal of effusion, or unless the gut was gangrenous.

Mr. Lentaigne had passed from one operation to another. He took some trouble to follow up his cases, and found that a good many of the earlier ones had recurred. About two years ago he had adopted Mr. Halsted's operation. He found it so satisfactory that he has performed it frequently since. Not one of these cases had recurred so far. He agreed with the President as to the value of lymph effusion, but thought that good stout silk sutures greatly helped to keep the parts in position. He never found the silk cause any trouble. Halsted's method would fail if the sutures were too far apart. Catgut was purely a temporary means of holding tissues together. In children a year old or less the hernia could often be cured by use of a truss, but when the second or third year was passed not much could be hoped for by this method, although it should always be tried first. He always took the sac away from the scrotum, and usually drained in such cases from the bottom of the scrotum. Previously he had had a case of a kind of hydrocele, and another, in which unfortunately sepsis had occurred, extensive suppuration had taken place in a large sac which was left behind in the scrotum. In draining at the scrotum or in any other place where the parts might be contaminated, he placed a good big piece of gauze charged with antiseptic material over the drainage-tube and stitched it to the skin by a row of catgut sutures about an inch all round the wound. At the end of five or six days the sutures were absorbed. Celloidin might be better than catgut sutures. It was more quickly applied.

Mr. Wheeler said that after the radical cure of hernia it was

not advisable to apply any pressure over the part, as pressure tends to cause absorption of the material thrown out, and so tended to produce a return of the hernia. Young children did not bear the operation well. He agreed with Mr. Bennett that many could be cured by a truss. He had a patient over forty years of age who declined operation, and who wore a truss for two and a-half years and was cured.

Mr. Gordon said that kangaroo tendon had the advantage over catgut in remaining as a support for the part for several months, and the advantage over silk in not remaining permanently.

Mr. Heuston, replying, said he agreed with the President that the cure did not depend on the sutures but on the plastic inflammation set up. That is why he recommended the twisted sac to be left through all the length of the canal. The weak point of Ball's operation was that the sac was cut off too close. Otherwise it was an excellent operation. He objected to silver and *silk* sutures, as months afterwards they might set up inflammation and suppurate out. He did not agree with Mr. M'Ardle in leaving the sac below. Colley, of New York, had reported 168 cases without one failure, operated on by Bassini's method. He recommended kangaroo tendon.

RESTORATION BY THE INDIAN OPERATION MODIFIED, OF A NOSE DESTROYED BY LUPUS.

By ROBERT H. WOODS, M.B., F.R.C.S.;
Throat Surgeon to Richmond Hospital, Dublin.

[Read in the Surgical Section, March 20, 1896].

KATE F., aged twenty-nine, suffered since before puberty from true lupus of the nose. For this she was successfully treated in the Richmond Hospital by my colleague, Sir Thornley Stoker, the treatment lasting intermittently over some years. She was submitted to the Koch treatment, and was one of the few who derived lasting benefit therefrom. The local treatment consisted in scraping the diseased area and cauterising the raw surface with acid nitrate of mercury. In the summer of 1894 the disease was finally cured and the patient sent to the country.

Before taking her leave she desired to have a substitute for the lost organ, and preferred a mechanical appliance to undergoing an operation. Her measure was accordingly taken, and a nose of the selected pattern was constructed to hang from the bridge of a pair of spectacles fitted with plain glasses. But the effect greatly disappointed her, and her disappointment grew into disgust when she found that, on stooping to scrub, the fickle ornament dropped from its position and mockingly flapped before her eyes. In addition to this there was, in the wearing of spectacles, a tacit implication of defective vision, from which she was most anxious to protect herself, while the incongruity of having to remove her nose in order to use her pocket handkerchief was very galling to her pride. She therefore rejected the apparatus.

with scorn, declaring that she would rather be without a nose than be troubled with so shabby a substitute.

Twelve months later (November, 1895), she again sought admission with a view to a plastic operation. There was no recurrence of the lupus; all the scraped surfaces were healthy. The operation was performed as follows:—A flap of skin (see Fig. 1) of a shape and size determined by a carefully adapted piece of gutta-percha, of Dieffenbach's pattern, was taken from the forehead a little to the left of the middle line, the left end of the incision was prolonged downwards so as to cross the bridge of the nose and end at the lower part of the opening at the right nasal cavity.

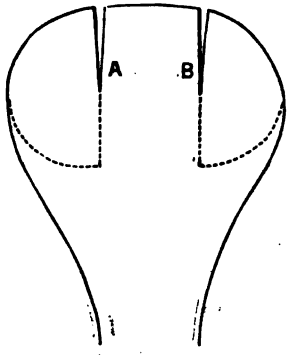

Fig. 1.

From the point at which this incision crossed the bridge, a corresponding cut was made down the left side ending similarly. The triangular flap between these latter two was then reflected from the bridge so that its raw surface looked forwards, its base acting like a hinge.

Two parallel incisions (A and B) were then made in the large flap, and the outer portions turned back, as represented by the dotted lines (Fig. 1), and coaptation secured by tiny leaden plates on the two skin surfaces connected by a horse-

hair. The square flap between the incisions (A and B) intended for the columella was then doubled on itself along its middle line, so as to bring its raw surfaces into contact, and secured in that shape by a horsehair suture.

Fig. 2.

The large flap was then brought down, the pedicle being twisted as lightly as possible in order to avoid strangulation; this was made easier on account of the flap having been taken from the left side of the middle line. To its raw surface the triangular flap from the bridge was adapted and fixed by two leaden plates with a horsehair between. A small surface high on the upper lip was freshened for the reception of the columella, and the whole flap, except a racquet-shaped surface on the right side, finally secured in its place by a large number of horsehair sutures. It will be seen that practically the whole of the inside of the artificial nose was covered with skin.

There still remained the raw surface on the forehead. To cover this, instead of drawing the edges towards one another, and allowing the interval to granulate and cicatrise as in the classical operation, a flap of skin of the same shape and size as the deficiency was taken from the left arm, carefully

avoiding the subcutaneous fat. The skin was put into warm sterilised water until required. This, though unattended by any bad result, was, I have since been led to believe, wrong in principle. Plain water has a destructive effect on tissues, and it might have been better had the skin been kept in normal saline solution or put in a clean dry vessel immersed in tepid water until required.

Fig. 3.

The flap when detached shrunk to less than one-third the size of the surface which it once covered. It was fixed to the forehead by about twenty horsehair points of suture. A small triangular depression which existed at its junction with the pedicle of the forehead flap was used as a drain for the fluid exuded; for this purpose a small wad of moistened cotton wool was put into the depression and renewed by the nurse every few hours.

No ligatures were used on the vessels during the operation. One artery in the scalp refusing to stop with forcipressure was secured by passing a loop of horsehair under it, through the skin, at a little distance from the wound.

Horsehair was the material used for sutures throughout, and it more than fulfilled our hopes; not a point suppurated,

and it left no trace in the skin. It had previously been sterilised by boiling for five minutes in 1 per cent. sodium carbonate solution. This had the effect of greatly increasing its elasticity without diminishing its strength.

It was thought better in mapping out the flap on the arm to take exactly the size of the surface to be covered, and not to allow for shrinking, because if a flap shrinks it must thicken, and there would be greater risk run of a thick flap dying than of a normally thin one.

No dressing was used except a piece of dry Gamgee tissue, laid lightly across the forehead, so as to keep the part warm, and yet allow enough ventilation to keep it dry.

Three days later the flap on the forehead was of a purple black colour, except at one point. It looked as if it were past recovery, but day by day its appearance improved, and it cleared up very much in the same way, and at about the same rate, as a black eye.

The sutures were removed on the sixth day and the lead plates on the ninth. The new nose united by first intention.

Needless to say the patient's appearance was greatly improved, but of this I shall allow the photographs taken before and after operation to speak. The paleness of the new flap on the forehead is due to the lighter colour of the skin of the arm, but when it has been exposed for a time to the sun and weather, it will probably simulate the rest of the forehead.

The second stage of the classical operation (division of the pedicle of the flap) was not proceeded with. It was found that the pedicle after about three weeks adapted itself perfectly to its position, while there was no necessity for utilising it to cover the forehead surface. The racquet-shaped surface on the side of the nose, which had contracted to a very small space, was excised after a month by an oval incision. This had the double effect of taking away a cicatrix and reducing

the somewhat parrot-shaped prominence of the bridge to more elegant proportions.

Fig. 4.

In operations like this contraction of the new nostrils has always been a great stumbling block in the way of success, so much so that some operators recommended that the columella should not be sutured, but allowed to hang down, so as to produce the effect of two nostrils, while in reality there is but one. My experience in this case shows me that this can be avoided. Contraction of the nostrils takes place in proportion to the quantity of raw surface left to cicatrise, especially if this be low down. If the inside be well lined with skin, and if immediate union takes place between the two layers of skin, then there will be, as in this case, little or no contraction. Hence it is of prime importance that the flap originally cut from the forehead should be of such a shape and size that the whole, or at least as much as is necessary, of the interior of the nose shall be coated with skin derived from the forehead. This, of course, involves a larger forehead flap, and up to the present surgeons have had to choose between the Scylla of contracting nostrils and the Charybdis of a too great scar on the forehead. Mr. Treves has thus been led in his operative surgery to advocate Langenbeck's pattern,

because the forehead wound can be better brought together, though it is obvious, from a look at Langenbeck's in preference to Dieffenbach's diagram, that there must be cicatricial contraction in the neighbourhood of the new nostrils. It will thus be seen that if we can overcome the necessity for economy in cutting the forehead flap, we shall solve the difficulty of great subsequent contraction of the nostrils. By transplanting the flap from the arm, the breach in the forehead, however great, can be easily filled up, so that this method has the double advantage of contributing towards the patency of the nostrils, and the perhaps more important entire avoidance of the scar on the forehead.

The President said that the patient was originally cured by him of lupus. She pressed him on several occasions to perform a plastic operation, but he declined as the parts had not cicatrised sufficiently. The chief interest he thought was not the mere reproduction of the nose, as much as the large transplantation of the skin from the arm. He thought it was too soon to be sure that there would be no contraction of the nostrils.

Mr. Bennett wished to know if any note had been taken of the sensibility of the skin forming the new nose. His experience was that sensibility returned about the 10th to the 14th day. It was at first dull. He had operated on several patients of the late Mr. Hamilton, in whom the nostrils had become occluded owing to cicatricial contraction. When the columella was not united to the upper lip, he found that there was no cicatricial contraction.

Mr. Wheeler had operated on about six similar cases. The great difficulty was to get union by the first intention. If that was obtained contraction rarely followed. The columella might slough from being bent too much. This could, however, be remedied by taking a V-shaped piece of skin from the upper lip and stitching it up. In Syme's operation flaps were taken from the adjacent portions of the cheeks and turned over. The patient must be a fat person to allow of this being done. He found small pins better than sutures in bringing the edges of the wound together. He never could satisfy himself about the sensibility of the restored nose.

In a patient operated on by Syme's method, sensation had returned on one side and not on the other. He wished to know whether a pseudo-mucous membrane developed on the inside surface of the nose, and whether the secretion from the nose scalded the parts.

Mr. Lentaigne said that Mr. Woods had modified the Indian operation by adding a Wolfe's operation of transplanting skin. It remained to be seen whether the new nose could not be made by some form of Wolfe's operation alone. Latterly he had transplanted skin a good deal and found it easy and generally successful.

Mr. Woods, replying, said he had tested the sensibility of the new nose. She always referred it to the place it originally occupied on the forehead, but he had not divided the pedicle and so probably sensation was conducted by some twigs from the supraorbital. The flap on the forehead, which had been taken from the arm, was absolutely wanting in sensibility. Practically the whole of the inside of the nose was lined by skin, and therefore no surface was left to granulate and cicatrise. He had not noticed any pseudo-mucous membrane. There was no secretion from the nose.

NOTES ON OVARIOTOMY.

By SIR WILLIAM STOKES,
Surgeon-in-Ordinary to Her Majesty the Queen in Ireland;
Examiner in Surgery, University of Oxford, &c.

[Read in the Surgical Section, May 1, 1896.]

In the evolution of Abdominal Surgery which has occurred since I commenced surgical practice, no measure stands out in better relief than the operative treatment of ovarian disease. I can well remember, when a student in Vienna, a gentleman attending the surgical clinique there, and who was a Professor of Surgery in Stockholm, being pointed out to me by one of the students, who whispered with bated breath, "He has performed ovariotomy twice!" I did not learn the results obtained in these cases, but let us hope they were successful.

The excitement that the early cases of Mr. Clay, Sir Spencer Wells, and Mr. Baker Brown caused, I have still a clear recollection of, as well as the keen interest that was taken in their work by the *Dii majores* of the surgical profession both here, in London, and on the Continent.

It was chiefly owing, I think, to the way the results obtained by some subsequent ovariotomists were too often put forward, or published, that the opinion or belief was formed in the public as well as the professional mind that the operation should be strictly confined within the narrow limits of an exclusive specialism; and some even went so far as to hint, in no uncertain manner, that the operating surgeon of a general hospital, who had the hardihood to cross the sacred threshold of gynæcological specialism,

was indifferent both to his own reputation and to the welfare of his patient.

It is hardly necessary for me to dwell on the complete change that has, of late years, come over the scene, and how, owing to improvements in various points in the *technique* of the operation, but especially to our greater familiarity with the principles and practice of aseptic and antiseptic surgery, as elaborated by Lister, the difficulties and obstacles which formerly surrounded the operation have, one by one, been swept away, rendering the pathway to success, in the great majority of cases, clear and reliable, and the operation one now frequently performed with signal success in most of our general hospitals.

The following notes are based on the results I have obtained in the last twelve cases on which I have performed ovariotomy. The operations date from June, 1877. Of these twelve cases, ten were brought to a successful issue, giving a percentage of recovery of over 83. It must, I think, be admitted that this result is, on the whole, encouraging, particularly having regard to the fact that the majority of the operations were performed in hospital structures of considerable antiquity, and some of them before many of the modern appliances and devices for promoting surgical cleanliness now constantly in use by every conscientious surgeon were introduced, and when the methods of maintaining wound asepticism during and subsequent to the operation were not as well understood as they are at present—methods which we know now are so essential, and without which the surgeon is seriously handicapped in his efforts to bring his operation cases to a satisfactory termination. The notes of some of these ovariotomies have already appeared in the *Dublin Medical Journal* and other periodicals, and the tumours removed in all the cases have been exhibited in the Surgical Section of the Academy. The last four cases,

however, that I have operated on have not been published, and, avoiding all minute details, I will briefly mention the more important particulars about them.

For the following notes I am mainly indebted to Mr. William Taylor, late House Surgeon to the Meath Hospital:—

Case I.—The first case was that of a patient, aged fifty-two, married, and the mother of three children, who was admitted into hospital on June 19th, 1892, on the recommendation of my friend, Dr. Fraser, of Drumkeeran, Co. Leitrim. She stated that she first noticed the tumour fourteen months previously to her admission. It was globular, fluctuating and movable; resonance was noted in both flanks, but the dulness observable all over the anterior portion of the growth, extending further on the left than on the right side. The circumferential measurements were, at the base of the chest, 31⅝ in., and at umbilicus 38⅜ in. The distance from the xiphoid cartilage to umbilicus 9½ in., and from umbilicus to pubes 10 in. From umbilicus to anterior superior spine on right side 10⅜ in., and on left side 10½ in. About six weeks previously to her admission into hospital the tumour was tapped, and a quantity of very thick fluid, like boiled starch, was drawn off. There was absence of pain up to the time the tumour attained its greatest size, when the patient experienced some "soreness" at its upper margin, three months previously to her admission into hospital. This continued increasing in severity. She suffered occasionally from nausea and vomiting in the morning. Menstruation ceased for about two years, but latterly there had been a slight return of it. For about eighteen months she had suffered from "dull aching pains in the loins," which were intensified when she was in the recumbent position. The patient had been married nineteen years, and had her last confinement twelve years ago. She had a miscarriage eight years ago. A second series of measurements were taken on February 2nd, and even in the short interval of time between the first and second measurements a distinct increase was observed. On this date I operated. There were extensive adhesions on the anterior and upper portions of the tumour, which, however, were easily broken down. I then removed 332 oz. of fluid, which was porter-coloured, viscid, and albuminous. The tumour, which consisted of two large and many small cysts, and weighed 8 lbs.,

was then removed, and the pedicle was found to be a long one. This was transfixed and ligatured, and after a careful toilet of the peritoneum the wound was sutured and treated with dry boric dressings. The progress to recovery of this patient was uninterrupted.

CASE II.—The next case was that of a young woman, aged twenty-two, who was admitted into hospital under my care on November 15th, 1894. The abdominal enlargement from which she suffered, she noticed first about twelve months previously. At that time it was more marked on the right side, and she suffered from much pain occasionally in the right iliac fossa. The swelling then increased slowly but steadily. Six months after the tumour first appeared she was advised by Dr. Fraser, of Drumkeeran, to place herself under my care, but she postponed doing so for another six months, at which time the enlargement had greatly increased and was as well marked on one side as the other. Menstruation was quite regular all this time. On admission into hospital the abdomen was found to be swollen up to the ensiform cartilage, and the enlargement uniform. In this case, as well as in most of my other ones, I had the advantage of Dr. Atthill's opinion as to the advisableness of operative interference, and at the operations the assistance of my colleagues, Sir P. C. Smyly and Mr. Hepburn. On Nov. 22nd I operated, making the usual median incision about 4 inches in length. On exposing the cyst I found that there were no anterior adhesions, either recent or old. I then tapped the cyst with a Wells' trochar and evacuated 130 ounces of chocolate-coloured fluid. Owing, however, to the existence of many smaller cysts, I found I could not remove the tumour without materially enlarging the wound. The pedicle was found to be a fairly long one, which I then transfixed and ligatured in the same manner as in the first case. As there was practically no hæmorrhage or escape of any of the fluid of the tumour, I did not deem it necessary to make any diligent "toilet" of the peritoneum, but closed the wound at once and applied dry boric dressings. The subsequent progress of this case was quite uneventful. The wound healed by first intention. One dressing only was applied, and the patient was up on the 15th day after the operation.

CASE III.—The next case was that of a married woman, aged forty-five, from Killicannon, Co. Cavan, who was admitted into hospital on August 7th, 1894, having been recommended to me by

the late Dr. Mathews. Twelve months previously to her admission she first noticed her menstruation becoming scanty and occurring at longer intervals. Three months after this she observed her abdomen becoming enlarged. After some months this enlargement was accompanied by severe shooting pains. These were relieved by tapping the tumour, which was done on May 15th, when 14½ pints of a dark-coloured fluid were drawn off. After a month the tumour was found to be as large as before. Since the tapping the monthly discharge has been entirely absent, but the pain has not been so marked as previously. The abdominal walls were tense, shining, and over each lateral aspect of the tumour veins largely distended were seen. The tumour was spherical in form, but bulging slightly on the right side, measuring from spina to umbilicus 21½ in. on the right side, and on the left 19 in. On a deep inspiration the tumour moved downwards ½ to ¾ of an inch. From ensiform cartilage to symphysis pubis measured 18 in. There was uniform dulness over the tumour, and some nodules could be felt in the umbilical region, and others extending downwards towards the left flank. The great bulk of the fluid contents appeared to be on the right side.

The examination of the uterus confirmed me in the opinion I formed that the tumour was distinctly ovarian. On August 15th I operated. The abdominal walls were very thin, and on exposing the cyst numerous adhesions were found. These, however, were easily broken down, and the cyst being punctured, 342 ounces of fluid were removed. The pedicle, happily a long one, was transfixed, ligatured, and divided, and there being no hæmorrhage or escape of fluid into the peritoneum, no "toilet" of that structure was resorted to. Deep and superficial sutures were then inserted without any drainage tube, and dry boric dressings applied. Like the last case, the progress to recovery was uninterrupted. The dressings were only changed once on the fifth day after the operation, when the wound being quite healed, the sutures were removed. On the twelfth day the patient sat up for two hours, and on the twenty-first day the patient returned home, since which she has remained in perfect health.

Case IV.—The next and last case I shall at present draw attention to is that of a patient, aged forty, unmarried, who came under my care on September 15th, 1895. She was a pale, sallow-coloured woman, with a somewhat idiotic expression of face, and a

very peevish, unhappy, discontented temperament. She stated that the Christmas previous to her admission she first observed the abdominal enlargement. It appeared first on the left side, at the lower part corresponding to the left iliac fossa, and was accompanied with a good deal of pain. As time passed on, the swelling increased until, on admission into hospital, the abdomen was greatly distended with a fluctuating tumour. Dulness was noticed on the front of the tumour and resonance over the right lumbar, but dulness over the left lumbar region. On the morning of the operation the following measurements were taken:—

From umbilicus to symphysis pubis,		7½ inches.
„ „	xiphoid cartilage	9 inches.
„ „	left anterior superior spine	8¾ inches.
„ „	right „ „ „	8 inches.
Circumference of abdomen above umbilicus		34½ inches.
„ „	below „	36½ inches.

The existence of ascites was recognised, but from the history and physical character of the enlargement, I held the view which subsequently proved to be erroneous, that it consisted mainly of an ovarian cyst. I do not think any blame should be attached to me or anyone else who examined the tumour for coming to this conclusion, for I do not see how, in the present state of our knowledge, it would be possible to recognise the condition of things that I found at the time of the operation existed.

On September 27th I operated, assisted by Sir P. C. Smyly, Mr. Hepburn, and Dr. Atthill. Mr. Croly and Mr. Ballance, of St. Thomas' Hospital, were also present. On reaching the peritoneum, which apparently was much thickened, and opening it, a copious gush of clear serous fluid took place, and this continued until such a complete collapse of the abdominal enlargement occurred that for a moment I began to apprehend that I had made an error in diagnosis, and that the case was not an ovarian one. However, on the cessation of the flow of serous fluid I enlarged the opening, and passing my hand into the abdomen I grasped the tumour, about the size of a large cocoa nut, and which was in the left iliac fossa, and with difficulty drew it forwards towards the opening in the abdominal wall. This difficulty was caused by the existence of numerous firm adhesions, which existed chiefly on the posterior aspect of the tumour, and also from the fact that there was practically no pedicle. A considerable time was spent in detaching these adhesions, effected chiefly by ligature and division. The cyst,

which was very friable and contained only a small quantity of fluid, was opened to ascertain more exactly its nature, and, if possible, from whence it sprang. The base of the tumour was then ligatured in several sections by a blunt needle, made much on the principle of the one known as Reverdin's. The cyst was then removed, and the cut end of the stump lightly touched with the thermo-cautery before being dropped back into the abdomen. The "toilet" of the peritoneum being carried out in the usual way, its divided edges were brought together by a continuous suture of carbolised catgut, after which the edges of the wound were united by means of five interrupted sutures passed deeply through the tissues. Boric dressings were then applied and the patient replaced in bed.

On the 9th day, at the first dressing, the sutures were removed, the wound having completely healed.

On the 13th day the patient was allowed to sit up for a short time, and soon after this she returned home.

I feel confident that one of the main causes of the success that, as a rule, now happily attends the operation of ovariotomy is the attention that careful surgeons pay to the preparatory treatment of the patient. This, according to Professor Ashton, of Philadelphia, should be carried out systematically for at least seven days previously to the operation, and consists of "rest, bathing, care of the bowels, regulation of the diet, special antiseptic preparations immediately before operation, and precautions against shock and vomiting." During this time the patient should be kept in bed, excepting, of course, when taking a daily bath, the water of which should be impregnated with an antiseptic such as eucalyptus. The frequent irrigation of the vagina with corrosive sublimate solution (1 to 4,000) is advocated by some surgeons, but is a practice that up to this I have not had recourse to. On the morning of the operation a thorough surgical cleansing of the abdomen is carefully carried out with soap, creolin, and ether, and a piece of lint,

folded twice and soaked in a solution of carbolic acid (1 in 40), is laid over the field of operation.

Another element in the preparatory treatment that is strongly advocated by Ashton is the hypodermic injection of sulph. of strychnin ($\frac{1}{15}$ gr.) three times daily. According to him this drug has a signal effect in preventing the occurrence of, or at all events diminishing, post-operative shock. The hypodermic administration, immediately preceding the operation, of morphin ($\frac{1}{6}$ gr.) is, I think, to be commended, and, unless some very decided contra-indication exists, chloroform is the best and safest anæsthetic, being the one least likely to be attended with post-operation nausea and vomiting, the disturbance caused by which militates so strongly against the satisfactory progress of the case. The diet for some days previous to the operation should be of such a character as to leave the bowels as empty as possible at the time of operation. This should be essentially of a sloppy character, and be coupled with a complete abstention from alcohol in *any* form.

As regards the *technique* of the operation I have little to say that is not to be found in the writings of Sir Spencer Wells, Prof. Ashton, Greig Smith, Mr. Lawson Tait, and other recognised authorities in connection with this operation. From my experience of it I would be disposed to advocate a tolerably free rather than a very limited abdominal incision. This is not in accordance with the views of Mr. Lawson Tait, who, I believe, is in favour of a very limited one. In Germany the pendulum swings in the opposite direction, a common practice being to remove ovarian cysts of considerable size without opening them, and through necessarily large abdominal openings. I would be slow to adopt this method, as I feel sure it must necessarily tend to augment the shock of the operation, and allude to it only from its having reliable credentials, and to point out that the length of the abdominal

incision does not apparently injuriously influence the results, which, as a rule, are satisfactory.

But I would say that damage to the edges of the wound would probably result from dragging large tumours, sometimes with solid contents, through them. This has been noted (W. T. Stewart M'Kay), and from *à priori* considerations one would say is likely to occur and slowness in healing follow—results usually observed after any bruising or contusion of a wound.

In reference to adhesions I would say that the number and firmness of them do not appear to me to militate against the ultimate success of the operation, always provided they be properly dealt with; the operator should carefully abstain from forcibly breaking down any ones that do not readily yield to a gentle pressure of the finger or gauze sponge. All the firmer and older adhesions should be carefully ligatured by chromicised or carbolised catgut previously rendered, beyond all suspicion, aseptic, and then divided with scissors. This is particularly necessary when the omentum has become attached to the tumour, that structure being exceedingly vascular. Prof. Ashton suggests a practical point in such cases—viz., in breaking up or dividing firm adhesions, to keep as close as possible to the tumour, there being then much less danger of wounding the hollow viscera. When the adhesions are firm and deep-seated in the pelvis, particular caution is required, as hæmorrhage from the anastomatic vessels between the uterine and ovarian arteries is not unfrequently a source of serious trouble to the operator and danger to the patient. The breaking down or tearing of such adhesions should be avoided. In some cases of this sort Ashton has recommended putting the patient in the Trendelenburg position, but of the value of this plan I cannot speak from personal experience.

In dealing with the pedicle we may now, I presume, regard

the extra-peritoneal method as of interest only in a historical point of view to the surgical antiquary, and the division of the pedicle by the cautery, as was formerly practised by Baker Brown, Keith, and others, may be relegated to a similarly deserved limbo. The ligature, which may be employed in a great variety of ways, is now universally adopted, and the material—strong silk—asepticised above suspicion. Gusserow, Martin, and other eminent ovariotomists employ multiple ligatures, but here and in England a single one is usually adopted. Cases, of course, will arise where the pedicle is very short and broad, or—as in the fourth case I have noted in this paper, where there was practically none—where multiple ligatures may be required, but in the great majority of cases a single one is sufficient, and for many reasons is to be preferred. Among these may be mentioned the undesirability of making several punctures in the pedicle, thereby incurring the danger of the formation of hæmatomata, and also that where there are many ligatures there is the danger of some of them not becoming either absorbed or encysted, and acting, in consequence, as sources of irritation and disturbance. To diminish the chances of the formation of hæmatomata, it is desirable to employ a blunt needle. The instrument I have for some time employed is one given to me by my friend and former pupil, Dr. J. Murphy, of Sunderland. It is made on the principle of a Reverdin's needle, but blunt-pointed. With this the pedicle is transfixed, the ligature caught, and each lateral half of the pedicle then tied. The pedicle is then divided.

The objections urged against the ligature have been based on the alleged danger of sloughing of the stump. The reasons that such apprehensions need hardly be entertained, are pointed out by Mr. Knowsley Thornton. They are that the vitality of the stump is provided for by a central capillary circulation, and also that vascular connec-

tions are established by means of adhesions of the stump to the neighbouring peritoneum, and also by the contact of the peritoneum at each side of the ligature favoured by the deep groove made by the ligature into the tissue of the pedicle. It has also been stated that the presence of the ligature favours a hyperæmic condition of the parts, which promotes the adhesive process (Fagan). I should be slow to say that these explanations are altogether satisfactory, but certain it is that if the ligature or ligatures be properly applied, sloughing of the stump is a calamity of which we need not be very apprehensive.

As regards post-operative treatment, it is hard to lay down any definite undeviating rules in consequence of the great variability in the way patients are affected by mental disturbance, before and after the operation, by anæsthetics, and the operation itself. Much, therefore, must be left to the surgeon's discretion, in the exercise of which he will be guided by experience and his own instinctive intuition of what is in accordance with the dictates of surgical common-sense. But, speaking generally, I would say that the rules laid down by Ashton should, in the main, be followed. For the first 24 hours, if possible, nothing in the way of food should be given by the mouth; after this peptonised milk in small quantities or koumiss, and later on chicken broth or carefully-made clear beef-tea; and for a drink, milk in iced soda or potash water will answer best in the majority of cases. For persistent gastric irritation what answers best is a small quantity of dry champagne well iced, or iced soda-water with a teaspoonful of brandy. All drugs should be avoided. In such cases rectal feeding should be resorted to until the tendency to vomiting ceases.

The cases on which I have based the preceding remarks are doubtless few in number in comparison with the stupendous statistics of this operation published by some practitioners.

Still the cases will, I think, strengthen the view I hold that the operation should no longer be regarded as belonging exclusively to the limited province of the specialist, but may be undertaken with confidence in the great majority of instances in a general surgical hospital, the hygienic surroundings being suitable, and the operator one who works conscientiously and faithfully, and is possessed of judgment and ordinary manipulative dexterity.

Mr. Fitzgibbon said that the best incision was one which would let the operator's hand into the abdominal cavity with ease.

Mr. Tweedy did not think that general surgeons should perform the operation of ovariotomy. All the tumours mentioned by Sir William were large. The larger the case the more difficult was the diagnosis and the operation. In none of the cases were the symptoms mentioned of any practical value as regards the diagnosis of an ovarian tumour. A fibro-myoma would present the same symptoms, as far as measurements were concerned. A fibro-myoma often lasted for years without any menstrual show. Was the general surgeon prepared to operate on a fibro-myoma in which there might be extensive adhesions to the intestine, bladder, and ureter? Was he aseptic enough? A general surgeon meets every day with septic cases, and had his hands bathed in pus. As regards the operation for ovarian tumour there was generally no difficulty in it when once it was diagnosed. The only way to diagnose an ovarian tumour was to feel the pedicle and the round ligament. The person who diagnosed the case should operate on it. The necessary asepsis could only be obtained in a room wholly set aside for abdominal surgery and not in an ordinary operating theatre. As regards the abdominal incision, if the tumour was cystic, by tapping the cysts an enormous tumour could be brought through a small incision. The larger the incision the greater the danger of abdominal hernia, though if the operation was strictly aseptic and no stitch abscess formed, there was very little fear of abdominal hernia. In ligaturing the stump the danger was not that the stump might die, but that the stump might become adherent to the intestine, and intestinal obstruction follow. A good precaution to prevent

adhesions was to suture the peritoneum over the stump. There was another danger, that the ligature might slip. The best knot to tie on a small stump was a Staffordshire knot, although even it sometimes slipped. Martin, of Berlin, ligatured the ovarian ligament as well as the stump. He wished to know how Sir William stitched up the abdominal incision.

Mr. William Thomson said the length of the incision entirely depended upon the size and character of the tumour. It was never desirable to make an incision longer than was necessary, and therefore he first made a small one and enlarged it as required. Sir William Stokes had rightly claimed that these cases should be operated upon by the general surgeons. Mr. Tweedy's claim was for the gynæcologist. But when it came to a question of septic hands, he wanted to know what position the gynæcologist held in that respect after his morning's work of examination of his cases. If a general surgeon was not fit to open a serous cavity because of the state of his hands, then he ought not to operate at all or touch a wound. Yet, in spite of this supposed inability to have aseptic hands, septicæmia had been practically banished from general hospitals. Mr. Tweedy said that the case ought to belong to the person who diagnosed it—meaning the gynæcologist. But this assumed that no one else could diagnose an ovarian tumour, and that a gynæcologist was necessarily able to operate. This whole question of diagnosis was abandoned when it was laid down that doubtful cases were to be determined by opening the abdomen. Anyone could diagnose under such conditions. This claim was an impossible one. The gynæcologists had been taught by the general surgeon, to whom was due the splendid developments in abdominal surgery.

Mr. Croly said that the only difficulty he found in ovariotomy was the diagnosis. His ovariotomy and hernial cases did best with rectal feeding for three days after the operation. He strongly condemned Spencer Wells' trocar and canula. Having made the abdominal incision, he punctured with an ordinary scalpel.

Mr. M'Ardle said that a general surgeon might cut into the liver, might remove the kidneys or spleen, but he was not to be allowed to touch an ovarian tumour. It was a great deal more dangerous to deal with a kidney than with an ovarian tumour. He had had to remove portions of the bladder and to interfere with the intestines and ureters, and had no calamity. He agreed with

Dr. Tweedy that a long stump was a great danger on account of its adhering to the intestines. He had to open the abdomen twice on this account.

Dr. Winifred Dickson said that a gynæcologist did not meet with much pus because the general rule was that the patient was douched before being examined. The vagina did not generally contain streptococci or staphylococci, and the gonococcus was not particularly dangerous in the peritoneum. If a case was suspected to be one of cancer it was not examined on the day of an operation. As a general rule gynæcologists operated early in the morning, before going round the hospital. She thought nobody used Spencer Wells' trocar now. One point about the after-treatment, not mentioned by Sir William, was that the sooner purgatives are begun the more quickly the patient seems to recover. A great many people gave a purge twelve hours after the operation.

Mr. Lentaigne said that in cases of laparotomy a great diminution in shock could be effected by avoiding injury to the peritoneum by a foreign fluid. He now always douched and irrigated with sterile normal saline solution. In suturing the wound he first sutured the peritoneum, and then the layers of the abdominal wall with silk, and, lastly, the skin with catgut. He then sealed up the wound with celloidin, and used no dressing except to prevent it from being rubbed by the bed clothes. The wound could be seen through the transparent celloidin. The celloidin was a great improvement in the treatment of strictly aseptic wounds. He thought it made no matter whether a man was a general surgeon or a gynæcologist, if he was a good surgeon he should do ovariotomy all right. The best means of asepticising the hands is with oxalic acid. If an operator opened the intestine, it might be easily as bad as if he had touched some pus, still he could make his hands clean and finish the operation. He could not altogether agree with Dr. Dickson in thinking the gonococcus of no harm.

Sir William Stokes, replying, said that Mr. Tweedy said that large tumours were difficult to diagnose. That was not in accordance with his experience. His fourth case was a very small ovarian tumour, and it was very difficult to diagnose. He also stated that unless the pedicle could be felt an ovarian tumour could not be diagnosed. A considerable number of ovarian tumours had no pedicle at all. He agreed with Mr. Thomson with regard to the incision. In Germany they made very large incisions, but it

increased the shock of the operation and the tendency to the formation of abdominal hernia. He always transfixed the pedicle before tying it ½ to ¾ inch above where the section was made, and he thought there was no possibility then of it slipping. In stitching the abdominal wound he did it the same way as Mr. Lentaigne, but without the celloidin dressing. If Wells' trocar was used with discretion there was no danger. With it there was less chance of the contents getting into the peritoneum.

SECTION OF OBSTETRICS.

BRIEF SKETCHES COPIED FROM MY BREVIARY

Of the Priceless Sayings of other Men, with some remarks showing what valuable outposts they were in helping me to extend the boundaries of my knowledge, and to avert death while practising the Obstetric Art.

By WALTER BERNARD, M.R.C.P., Londonderry.

[Read in the Section of Obstetrics, November 22, 1895.]

As the progress of time gradually set my midwifery practice aside, other medical subjects have of late years more fully occupied my thoughts. But as I have finished my work in this particular branch, duty calls me to give an account of my stewardship, as I am no longer steward. Each man busily engaged in active practice ought from time to time to have himself weighed in the balance, so that, if he be light-weighted, the actual weighing will act as a stimulant to carry him up to a certain standard. We were all examined, but we should all present ourselves again and again for re-examination. My examination, as well as that of others, was a very superficial *vivâ voce* one, and if it had not been superficial, as a matter of course, I should have been rejected, as my knowledge was the merest smattering.

But what saved me as years rolled on? Simply the truth of the old proverb being clearly proved to my mind, that

"writing maketh an exact man." What preserved me from many a danger were written records.

The copyist and colourist, Sir Joshua Reynolds, said that "reading the thoughts of other men made observers." More than this, the Harveian Oration year after year animates us to imitate our benefactors.

I show my breviary to-night, as a few glances at it, however imperfect it may be, will illustrate more fully the plan adopted. My plea for working under this method is that it made my observations more accurate; and that such collective wisdom taught me more forethought, more patience, more promptitude, and more readiness of resource. These records are held together with as much precision as is practicable, and without the loss of time and weariness of research information is at once reached, for, as we have all experienced, the busy man has not time or inclination, when tired, worried, or anxious, to search in books of reference. An indexed system somewhat after this fashion, jotting down from month to month a few sentences, will ultimately reach to such a pitch of excellence that it will give the utmost satisfaction to the man who adopts it. Memory is dangerous, and ought not to be trusted. Want of time cannot be an excuse for not recording, and, as Gowers says so truly, "Shorthand, which is now substituted for longhand, can secure some precious words of wisdom." The more the precious thoughts of other men are kept constantly before us, the more they are emphasised, the better we become, and knowledge comes upon us as by stealth—

> "As the morning steals upon the night,
> Understandings begin to swell."

They are great outposts, and help to expel self-life in living a daily midwifery life.

To a man launched into the world to do battle against

death, midwifery is the highest branch for imparting teaching. It arouses the perceptions and energies into activity by mechanical principles. It teaches him sympathy, honesty, self-denial, patience, and forbearance; and many indeed there are in its ranks whom it has taught to rise above the level of all that is acrid, trivial, and temporary in self-seeking, commercial thoughts, and reputation.

No man should have it recorded in a friend's note-book that he ever refused to go into the lowest slums to attend at night or day in difficulty and danger. In this science a man must be so physically and mentally constituted, that he must be ready in an instant for any kind of work. If this be so, then before we enter its ranks the responsibility must and ought to be realised—our personal fitness, personal inclinations, personal circumstances. Do we go to it for a living, for reputation, or for commerce? Shall we give our bodies, souls, and spirits to its entire devotion?

The training of the Dublin School during Montgomery's time had a refining influence—due, in a great degree, to his being an accomplished scholar. The education he sent forth by his classic work on the symptoms and signs of pregnancy lessened many sad events. His book was eagerly studied, and he taught many a practitioner to lift off the clouds of gloomy anticipations from the depressed minds of pregnant women—not only insisting more on the use of purgatives, but also on the importance of reassuring words. From what I have since learned, I doubt not, these two factors saved many a woman from puerperal fever and mania. It is not out of place to mention here that Montgomery, while collecting material for his work, gratefully acknowledged the assistance he received from the late Dr. William Daniel Moore, of Dublin. Churchill's book on "Diseases of Women" followed, and, being more general, was a ready help to practitioners.

PUERPERAL FEVER.

My review of this subject, called up from the experience of other men, and taken from my records, will give a short sketch of its many phases.

Strother, of London, was the first to give it that name in 1718, in a published Essay on Fevers, and Manning, in 1771, wrote as to its cause and nature, but its lore and complications, from the time of Hippocrates, were manifest.

Smellie, who is a lesson to us all, had much of it in the homes of the people. In 1734 he studied Chapman's and Giffard's works carefully, and was a great compiler and note-taker. Whatever he advanced of his own was from real facts and observation. He was no selfish "secret-monger." His philosophic mind was exhibited by the candour with which he confessed the errors he had committed through want of skill and judgment. Though he has taught us many lessons, we have yet much to learn from him, who, so to speak, did not, like many of us, fossilise experience, failures, or disasters. Smellie, though he studied puerperal fever—John Hunter like—did not know really what it was; and I now state, that even so late as up to 1860, it was jumbled up with other puerperal troubles.

A disease, *sui generis*, was unnecessary. But we, who were actively engaged in midwifery practice, well knew, from sad records of our own, that erysipelas, typhus, scarlatina, diphtheria, the dissecting-room infection conveyed from the operator's hand while trying to remove pieces of membrane, and the *post-mortem* table, were factors from which we obtained materials for originating puerperal fever. The absorption of these materials was too often favoured by a relaxed state of the uterus and *post-partum* hæmorrhage. During these times the whole mortality of England, as we are told, exceeded 3,000 annually, and it was not until Sir William Jenner aroused men's minds to

the foci of contagion that the real practical work commenced. There was no mention of vaginal injections in the epidemics of 1827 to 1832 in Paris, nor in that of 1842 and 1844. Skinner, Physician to the North Dispensary of Liverpool, was one of the first to advocate the use of disinfectants with the lochia, and Graily Hewitt and Tyler Smith followed, laying great stress on disinfectants and antiseptic precautions. This was in the sixties. I need not give any abstracts from the many discussions on puerperal fever before the great one in 1875. It was this discussion which opened up the way to a clear understanding of its nature, and I must say that many other diseases were mixed up with the name in my mind, and, I am afraid, in the minds of others too, until this discussion cleared away the approaches to something more distinctive under that head.

So much did that great meeting impress me at the time, that I have here, on it alone, extended notes of what was sent forth. Vast material was opened up in its relation to infective diseases. Many took stock of the knowledge for elucidation, and since then it has been sifted by careful and accomplished hands. In 1882 Barnes said that the heterogenetic commences in 24 hours. This was a great point to decide for many reasons—one is that a nurse threatened to bring an action against a doctor for stating that she brought it to a case, the symptoms having commenced soon after delivery with the forceps.

The statements made by Dr. Atthill at the August meeting of the British Medical Association in 1883, coming from such authority, carried much weight. Though Dr. Atthill was announced in the programme to open the discussion, to the disappointment of many he was not present, but his address on Metria (so-called puerperal fever) was read, and subsequently published.

Here it is unnecessary to go over his views on the pathology of metria, but only to mention one or two important points of prevention in the two forms he described—of self-infection, and the epidemic.

His antiseptic precautions of washing out the uterus, and exciting abiding contraction in a relaxed condition of the uterus with ergot for a week after the termination of labour, became widespread in the minds of practitioners both in England and Ireland, and of this knowledge I had personal experience, as I knew, and still know many, who, though bound by antiseptic law to carry out the former, continue to have faith in the ergot plan also.

At the same Section Dr. More Madden said its entity was not affected by name. He was convinced that it was a specific fever, and that it had a distinct existence. He mentioned his views of the causes which still hold good. He differed from Dr. Atthill, and was convinced of a specific infectious disease peculiar to puerperal women, the violence of which, according to Dr. B. Sanderson's experiments, and according to the clinical experience of the Dublin Lying-in Hospital, was mild at first, and of intensified virulence after some time. Dr. Madden also showed that in the Dublin Lying-in Hospital the mortality was five times greater than extern deliveries, putting down hospitals as habitats from which, under favourable conditions, puerperal fever radiated.

He then spoke of the causes, and of Dr. Williams' suggestion in 1874 of disinfecting clothes. But long before this personal cleanliness and disinfection were known, somehow practitioners were not fully alive to the life-giving and life-saving importance of carrying out protective measures, and taking guarded care to see that all discharges were disinfected and moved on.

These opinions were strongly laid hold of, and acted

upon by many known to myself at that time in active practice. But what came on myself more than once by surprise were the insidious and late causes of puerperal fever, and their misleading symptoms, putting one off his guard, some ending in the suppurative and plugging process. The occurrence of obscure rheumatic pains, feeling cold, with little rise of temperature following, and slight hysterical symptoms, were to a great extent disregarded until Priestley roused men to their great danger in his Address read at Cardiff in August, 1885.

Treatment since then, in many cases by abdominal sections, has not justified itself. When the septic process begins in the uterus and vagina, and pervades the whole system, other tissues besides the peritoneum become involved, and drainage of the peritoneal cavity will not suffice in this general affection, which will kill without pus. And the removal of the supposed septic foci has not been in such attended with success. But, when due to pre-existing disease of the tubes, the treatment is most rational.

This scourge of ages has been almost stamped out, though some are yet reported as carriers of it—not doctors, however, but midwives. As regards precautions, there is too great a tendency abroad to magnify the power of antiseptics while losing sight of the fact that in the absence of suitable culture beds bacteria cannot live. Some years ago I learned this golden point from the lips of Dr. Barlow, while going round with him in the Great Ormond-street Hospital for Diseases of Children.

ECLAMPSIA AND ALBUMINURIA.

The knowledge we have gained of this subject since Galabin published his views in the Medical Journal for October 30th, 1880, clearly bears out what he then wrote—that albuminuria of pregnancy, and its relations to puer-

peral eclampsia, form a small portion of the whole subject. His grouping in this Address as to the causation of albuminuria ought to be carefully studied and well remembered.

From a clinical point, I believe there is a constitutional condition called into action by pregnancy, and, looking at it from a family-history aspect, a toxic agent exists somewhat akin to that which produces tonsillitis, pelvic cellulitis, and rheumatism. As regards treatment, it must have been as refreshing to old practitioners as it was to myself, who bled rather often, when M'Clintock sent forth that the lancet had been too much banished; and I see, under certain conditions, Drs. Barnes and Atthill still believe in bleeding. Albumen has been found in the urine of pregnant women with their first children, and mothers and children have got on well. In these we do not fully investigate the clinical side, and separate the apparently normal from the abnormal forms, which produce death towards the termination of utero-gestation in seemingly healthy women.

In future pregnancies there may be recurrences of death in utero, and yet healthy children coming to the full time, born alive, may follow. Many cases are now saved when albumen is detected by rest in bed, a milk diet, and elimination from the bowels. This mode of treatment frequently causes the albumen to disappear, and saves the necessity of bringing the gestation to an end. Despite the dictum of so distinguished an authority as Dr. Barnes, I cannot see the necessity of always carrying out what he lays down, that "whenever we find marked albuminuria, with or without convulsions in a pregnant woman, the first and most pressing indication is to bring the gestation to an end." A storm of flooding has been known to remove albumen and threatened convulsions. Why not imitate

nature, and bleed before having recourse to emptying the uterus? But Dr. Barnes says, if we do not do it, nature will produce conservative abortion. His golden rules were late in coming for me, and new in my time, though when they did come we held on to them. They are so good that they could not be repeated too often. He says, when a fit is probable make no examination, pass no catheter, and force no food or medicine until the patient is under the influence of chloroform. These rules, I am sorry to say, some forget. Now, as to chloroform, I never had any fear in giving it myself in midwifery practice, nor had I a fear of even a bungler giving it; but when administered apart from labour cases by some men, while I am standing by, I am in terror, though I cannot explain the reason why. Happily, however, I never witnessed a death from it, though some patients were apparently dead.

Before I close this division of my paper, I beg to suggest to thinkers and writers that Hughlings Jackson's "Hard Sayings," and Gower's "Dynamics of Life," be carefully studied, with a view to obtaining a deeper knowledge of eclampsia and its pathology. Looking at it from a clinical point of view, Sir Andrew Clark, in a short paper, deemed albuminuria to be in some of physiological origin, and this is why mother and child occasionally escape in pregnancy when albumen shows abundantly in the urine.

Though I have tabulated in this book some of Hughlings Jackson's great thoughts and work, as well as in one of the books in the asylum some years ago, I am free to confess that, after much labour, I have not taken him in in many of his complex points, though I read him over and over again. As to Gowers, his "Dynamics of Life" are not so difficult to follow, but it takes your full energies to fix your thoughts and arrest your attention, so as fully to appreciate the usefulness of his work.

SO-CALLED FUNCTIONAL OR ORGANIC HEMIPLEGIA.

Other disturbances of the motor area before and after delivery we occasionally come across. Many of the so-called functional are curiously alike, and give evidence by pallor, weakness, staggering, or flushing, of vascular and cardiac changes, and show slight symptoms of sensory, motor, or intellectual disturbances of a passing nature. I have known a few on getting out of bed in the mornings to be suddenly seized with a sense of weight in the left limb and side, with numbness, tingling, and dragging, but with no loss of tactile sensibility.

To all my cases, functional and organic, I can now look back and trace a decided history of the toxic agents of neuralgia, gout, and rheumatism. To a few, in the cases themselves, I could trace a history of chorea in childhood. Here Lewaschew's experiments are clinically verified—"that irritation in nerves leads to early changes in the vessels which they supply." In not a few of one's old cases can be found signs of commencing atheroma, showing that the vessels are older than themselves. And some of the most unexpected disturbances may be probably due to minute detached particles plugging small vessels in the brain.

Churchill's recorded cases of loss of power, before and after delivery, in his book on diseases of women, are of much practical interest to those who are still engaged in this study.

DYSMENORRHŒA.

Life-histories, too, show that dysmenorrhœa has a kinship to the toxic agencies which arouse mild and passing forms of loss of motor-power. It is not my intention to dwell on this subject, but merely to say that too much was said on local treatment during the discussion on Mrs. Garrett

Anderson's paper, read by that lady at the Edinburgh meeting in 1875. Doctors Barnes, M'Clintock, and Atthill spoke and lauded her local views. But the greatest difficulty is to arrive at a clear idea of the ætiology of dysmenorrhœa, as an essential preliminary, before we have recourse to local measures. I remember after that discussion was over, the practitioners, when they came out into the courtyard from the meeting, had local causes and measures on the brain.

PLACENTA PRÆVIA.

Permit me to remind you that Smellie did not follow any one method. He acted according to circumstances. Ramsbotham says, Smellie was the first to adopt the practice of perforating the placenta; but Smellie does not state himself that he was the originator.

The advice to make an opening in the placenta was known even before La Motte's times, and when the unyielding os forbad an entrance to the hand two alternatives at that time were practised—either to plug and give time for dilatation, or to detach the placenta from the lower segment. This lower segment calls up Barnes' Address, published in March, 1888, and read at the Section of Obstetrics at the Annual Meeting, in this city, of the British Medical Association, in August, 1887. He illustrated, as you remember, by diagrams, his circles and three zones. Many of us are aware that he put things in a clearer light, and held in check "the rapid and forcible methods." His views became widespread, and seemed to show, as he himself stated, that there was a large saving of life to mothers and children, and concurrently with a large saving of mothers, 33 per cent. of children were born alive. After Dr. Fleming read a paper at the Obstetrical Section in Dublin, in November, 1886, Dr. Kidd said that if hæmorrhage was extensive, and the os sufficiently dilated,

the first thing was to turn. Having done that, the best thing was to wait. If the membranes could not be reached and ruptured, introduce Barnes' bags. Where there was a complete presentation of the placenta, and it was found difficult to rupture the membranes, the placenta should be perforated. As exhibited, I here give you proof from my Breviary how much I, in common with others, reverenced anything that came from Dr. Kidd on this and kindred subjects. The old dogmas were held too long, and did not keep abreast with the well-timed changes. I need not enter into details here; but plugging and rapid delivery ought to have been long put a stop to before they ceased. The plugging ought to have been in many cases done away with, and, as Dr. Kidd directed, Barnes' bags used. I see the very reprehensible method re-introduced of dilating the cervix with the fingers in the Medical Journal for October last.

USE OF FORCEPS.

Forceps, like crania of the past, are of great archæological and anthropological interest, in that from the Pompeian times they are the means—in the most extended sense, from a Lombroso point of view—of exhibiting the lines of thought of men co-equal with their cranial formations. The specimens, though rude, point to much intelligence and adaptivity of means to an end, and the varieties in the museums are full of meaning. And, in a sense, the forcipated past holds the present firmly in its grasp. This history of the past, and study of the near past and present, help us to look forward with hope for even yet greater achievements. The near present has given us much hope in the induction of premature labour at the seventh or eighth month, or division of the symphysis in pelvic contraction and deformities.

In days gone by it was instructive to study the records of Ramsbotham in the forties and fifties, when he was attached to the London Hospital in the East End, and those of Lee in the sixties which followed, when he was attached to St. George's Hospital, as from their sad experiences we learned how to think and what to avoid.

Lee's dictum was pronounced on the frequent and early use of the forceps. He not only used them himself early and frequently, but he made many a practitioner smart and wince under his lash, when his advice and assistance were needed, for not having applied them earlier. He pursued the wrong course, for he as well as others did not know the right, but when he sometimes found that he was rather too precipitate in condemning a brother, he apologised, and there lives yet one of our most distinguished members to whom he did so, unsolicited.

The rules of interference varied much. In M'Clintock's high and low forceps, the high was put on too often and too soon. Simpson's rule was:—Do not use the forceps until the os uteri is fully dilated.

The rules of interference of Collins, Ramsbotham, and George Johnston, represent three distinct schools. Collins and Ramsbotham did not use them often enough—the former, to a great extent, substituting the perforator. Bailey belonged to Ramsbotham's school and did not apply them often enough, but Johnston went to the opposite extreme and used them too frequently. There was a good deal to be said on the side of Collins and Lee. In their days it was well known that ergot acted injuriously on the mother and child in a mechanical way, hence their onslaught; but still the administration of ergot went on until M'Clintock came to the rescue. His fiat went abroad, with much good results. It was this, that unless the child was born a short time after its action, it was very likely born dead. How

often the forceps were used by these different authorities, I need not here say; the numbers are well known, but, in passing, I may mention that one man used the forceps in one out of every ten or eleven cases, another only in one out of a hundred; and not fifty years ago Denham's law was again fiated—to wait until the child's head had rested six hours on the perineum. I was frequently advised myself to wait for four hours—and oh, with what desperate anxiety I used to pull out my watch to see if the time had arrived—but truly, I did not at all times wait until the four hours were completed, for, even in those days, it was well known that when the signs set in of flagging powers interference was essential to the life of mother and child. Owing to the frequent use of the forceps the lacerated and abraded passages called for another system. Turning followed, and instrumental delivery was held back again, but for a time only, by many arguments; the chief plea being, if version be safety to the mother and child where abnormal complications darken the prognosis, imparting difficulty to the execution, will not the advantage to the patient be increased where the obstructions are only normal? In Johnston's time, and in M'Clintock's time too, the forceps was put on so often that there were many severe lacerations of the cervix, as well as the perineum. The laceration of the cervix was not frequently found out at the time, even when it set up a little cellulitis, but in the near future the examination for endometritis brought it into view. If not endometritis, prolapsus uteri.

In Churchill's time I remember removing many incrusted wooden pessaries embossed with salts.

I hope I may be found out to be wrong, but I am of opinion that the bleeding, during climacteric storms, is more severe in cases with lacerated cervices.

But in studying the use of the forceps, we can only say

that we leaped to principles of the wildest kind, for in obstetrics, as well as in every other science, our senses ought to have been frequently appealed to. Had we relied more on our own personal experience, and not relied too much on that of others, we would not so often have gone astray. Turning over the writings of those whose tentative hypotheses through our ignorance we were not able to weigh in the balance, and their tentative character *not* being realised, we took their opinion for granted, and landed in pit falls, from the want of that exact reasoning necessary to solve complex problems in our moments of intense anxiety.

Haughton's calculations on the force exercised by the heart's action called forth from others calculations on the driving power of the uterus against the *contre coup* of *the* pelvis. Barnes' mechanics were followed by Galabin's. But it was the German sifting of Haughton's views on the heart that gave impetus to the investigation of these mechanical niceties. Thus in these days the prophylaxis of skilled mechanics and appliances have acted as preventive measures against hasty conclusions, lacerated soft parts, and old pelvic troubles, so common in the past.

FORCEPS AND LACERATION OF THE PERINEUM.

The following were some of the causes, in the past, of laceration :—Driving power of ergot, and forceps not drawing the head slowly away from the perineum; not removing the blades one by one gently in the axis of the outlet; not holding back head when practicable—now occiput, now chin—and forgetting to assist the natural alternate lateral movements of the head by the well-known pendulum method, taking guarded care to carry the forceps well forward and upward when the occiput engaged the outlet, and at the same moment pushing forward the perineum.

In my day, these rules were not so well thought over and so well taught and known as they are now to every young doctor, hence the more frequent lacerations in the past from this cause alone.

Sewing the perineum was seldom done, for two reasons—first, we tried to hide our want of skill from the patient and the friends; and secondly, when we sewed it, it seldom healed from being done improperly and from the want of sanitation.

In these days a ruptured perineum, if skilfully and antiseptically treated, is almost sure to heal. In sewing, the great object is to include the deeper tissues; if this is not well done, failure will be sure to follow, no matter how sedulously the nurse carries out the antiseptic laws. Tying the thighs together I never practised. I do not agree with some, that a repaired perineum is more liable to split than one that has not been repaired. I have watched this carefully, and have not found it to be so. Clearing out the rectum with hot water enemas, I believe, has much to recommend it and is not as frequently practised as it should be, as one of the preventive measures against laceration.

RETAINED PLACENTA.

So far back was external pressure used that it was unknown when, in remote antiquity, it commenced. Patience in the vanished past, as well as in Guillemeau's time in 1609, was recommended, but I could find no illustration, of it in Pompei or in the Museum of Naples. Nor indeed is it accurately known when the treatment of the "Dublin Method"* became fully established. Mr. Dease. in 1783, alluded to it; in 1848, Hardy and M'Clintock's observations

* To much that is original we are indebted to Dublin. Sir Fielding Ould was the first to establish, in 1742, that the head of the child does not enter and pass through the pelvis in the same position.

give a minute description of it. This method, if it did not succeed, brought the edge of the placenta within reach of the finger, and thus enabled us to hook it away. They did not insist on the complete extrusion of the secundines from the maternal passages by manual pressure, except in cases of hæmorrhage. Credé, of Leipzig, brought this method much under the notice of the German School in 1860, insisting on all being cleared away, by pressure effects, from the uterus and vagina. His description of the mode in which external pressure ought to be employed was pronounced. But how long we should wait before manual extraction has "many a time and oft" been discussed. Not very many years ago, pressing off the placenta had its advocates in Dublin and other parts, but I am happy to say the fashion lasted only a short period.

POST-PARTUM HÆMORRHAGE.

Dr. Madden's words in his Address at the Section in Cork, August, 1879, have been since that time more than verified. He said, owing to the causes having been more fully looked into, this lamentable accident has gradually become less. It is of vital importance that these causes should be frequently called up from our breviaries, as our memories are too treacherous, and as the lives of patients, in a great degree, depend upon how we deal with the particular cause. This is my plea for copying a few here. Most of us in country places will be forearmed and forewarned by the family and personal history, a knowledge of which will make us acquainted with some of the predispositions, such as hæmorrhagic diathesis, kidney disease, alcoholic disorders and congestions owing to liver, heart, and lung affections. When hæmorrhage does come, we must endeavour to know quickly the cause: if from partial or whole detachments; if partial, whether physiological or

pathological, as we are aware both of these require different methods of procedure in dealing with the placenta. I shall only here recall, for the sake of a little practical illustration, a few of the causes of *post-partum* hæmorrhage, such as dragging at the cord, rent in the cervix, adherent piece of placenta, portion of membrane left behind, and the revival of the precipitous method of crushing off the placenta. The general predisposing causes may be put down to delayed labour, hæmorrhagic diathesis. Bright's disease, alcohol, luxury, too rapid emptying of the uterus, multipara, producing over-distension and obstruction of the portal circulation. In *post-partum* hæmorrhage and placenta prævia especially, and indeed in all other difficult cases of midwifery, we are continually and incessantly standing on the juts and perilous ledges of life and death. If this be so, help ought to be sought early, in difficulty and danger, from the nearest practitioner. I say emphatically the nearest, for the delay in sending for our so-called trusted friend at a distance might cost the patient her life. In such a crisis let us hold in the utmost contempt our mutual distrust. What about being placed at a disadvantage; what about reputation and wretched fees? Let us risk everything, even our lives, in endeavouring to save that of our patient. You, who would get out of the way of a child on the footpath, and help her to the tenderest and most nutritious food at the table, can you then listen to a daughter's cry—"Save my mother! O, save her at any cost!"

If we do not look at it in this sort of way, let us look at it from a more selfish one—from certain facts of our own sensations, which we call consciousness; these may arouse feelings that we have left undone those things which we knew we ought to have done. If we realise this, it is possible we

may awake with a groan in the small hours of the morning:

> "Thus conscience does make cowards of us all,
> And thus the native hue of resolution
> Is sicklied o'er with the pale cast of thought."

In 1862, soon after Barnes published his results on the injection of a solution of perchloride of iron, and some time before Snow Beck vehemently condemned the treatment, I employed it, and injected into the vagina only. In a well-known case in 1866, which baffled every other plan, I, with the assistance of the late Dr. White, of Londonderry, successfully injected it in solution into the uterus. When this was done the lady was pulseless and in an extreme state of collapse. It was afterwards used in Dublin, and recorded by Dr. Atthill as being used in Ireland for the first time by Dr. Kidd. The late Sir James Simpson used it as a styptic very much mixed with glycerine. I saw him do so with one of my patients. Dr. Madden recommended the solution to be applied by means of a sponge. His plea for giving opium was not sufficiently considered, and the importance of his full and frequent doses in the well-known causes of his trickling cases could not be over-estimated.

Before I leave the perchloride, I may mention men were in doubt and afraid to use it freely until Dr. Kidd's important statements were published, testifying that it was a safe and sure remedy. Besides, Dr. Atthill gave it as his opinion that when injected into the uterus it did not penetrate into the veins. Dr. Braxton Hicks, too, very soon recognised its value.

I will pass by Hardy's binder and external manual pressure, and come at once to transfusion. Dr. Atthill's inferences, in 1877, and Dr. Purefoy's account of a successful case in June of that year, tended much to encourage others to practise the art.

Soon after this, in a case of triplets, I assisted Dr. Byrne, of Londonderry, to inject defibrinised blood from the husband, but the woman died. The hæmorrhage in this case had not ceased, and consequently bore out M'Clintock's opinion that transfusion should not be tried until bleeding had ceased.

In collapse from *post-partum* hæmorrhage, Dr. Macan's views, in 1876, opened up Hecker's (of Munich) plan of injecting ether in various forms of collapse hypodermically, and since that time its use has been established in extreme collapse. The treatment helped me much in cases that were attended by unskilled midwives, who knew neither how to grasp the uterus properly, nor to apply a binder efficiently. I only regret that I had not known its value years before—brandy and ergot alone being then my chief weapons, taking care to douche with cold water also. Now I only hope every man's syringe is in good working order, as one is, even in these days, liable to meet now and then with *post-partum* hæmorrhage. A new help is a revival of saline transfusions, of which I have no experience. I hope my statement will not be considered egotistical, when I say that though I had many cases at death's door, I never lost one which I had full charge of from the onset. This is, in a great degree, a mere coincidence, but at any rate it helps to show that to be forewarned is to be forearmed, for the main indication is to investigate what may be a likely cause of a coming storm.

Of the concealed accidental hæmorrhage described by Dr. Braxton Hicks I had no knowledge. Dr. Purefoy's paper, of 1884, in the Obstetric Section, as to the causes of secondary hæmorrhage, was of the utmost value to practitioners, as was the opening Address at Cambridge, in August, 1880, on uterine hæmostatics, giving the most common causes of hæmorrhage from the unimpregnated uterus.

Dr. Atthill gave a wide field for accurate diagnosis, and enumerated—

1st. Cancer.

2nd. Tumours of uterus.

3rd. Imperfect involution after labours and abortions.

4th. Granular erosion of the cervix.

5th. Granular condition of the intra-uterine surface; and

6th. Retention of a portion of the ovum after abortion.

Since I gave up this branch much light has been shed on uterine hæmorrhages, and so late as August, 1894, Dr. Smyly read his experience before the Obstetric Section of the meeting of the British Medical Association held in Bristol that year. He not only imparted information *that* was new, but elicited from Dr. Barnes and other distinguished men lines of demarcation to go upon. One remark of his I beg to call up—that easy dilatation meant easy laceration, the cervical tissues giving way like wet paper.

My own experience of placenta prævia is that it is a terrible disaster. In some the uterus was emptied too late; in others too early; in a few, after sharp hæmorrhage, mother and child did well; and again, when I, as well as others, least expected it, fatal results followed to one or both.

ENDOMETRITIS.

Very much in the fifties and sixties, agents were employed which were supposed to be capable of effecting good results. But the haziness of the causes of endometritis hung over us to a great degree until the discussion at the Cork Meeting of the British Medical Association, in August, 1879. Dr. Playfair commenced the discussion, and Dr. Atthill figured the causes, and spoke of employing occasionally, instead of nitric acid, iodised phenol and carbolic acid in solution. But prior to this many agents were employed which were supposed to be capable of effecting good

results. Edinburgh was the head quarters of gelatinous pessaries, and from there the infection spread far and wide. In 1868 I was there with a patient under the care of Sir James Simpson, and while in Edinburgh I went to see how these things were manufactured by the thousand in a well-known establishment.

For the sake of illustration the following is an example of how much treatment was carried to excess:—One of my patients had the os touched with iodine only over thirty times; next, the cervix was slit; third, caustic sticks of zinc were forced up through the cervix; fourth, another application, I do not know of what; and fifth, I took upon myself the responsibility of having the cervix dilated, and the interior of the uterus touched with nitric acid according to the old rules of Dr. Atthill. After much suffering and much torturing, the lady got quite well soon after the acid application. It was no wonder that Dr. Matthews Duncan, at the Manchester meeting in 1877, condemned excessive meddling. I remember in what a supercilious tone he spoke of the abuse of supports. Then Sir Russell Reynolds, the present President of the College of Physicans in London, in an Address on Specialism, on October 15th, 1881, said:—"Set fashions of disease they have treated or maltreated by endless examination, speculations, applications, and the like." And in his paper on Neuroses of the Viscera, published in the Journal of the 15th of March, 1884, Dr. Clifford Allbutt followed by stating that gynæcologists find a neurotic woman's uterus like a nose, a little to one side, flabby, and running. It is impaled upon a stem, or perched upon a prop, or is painted with carbolic acid every week, except during the long vacation, when the gynæcologist is grouse shooting, or salmon catching, or leading the fashion in the upper Engadine.

As far as my experience goes these conventionalities

were not carried to excess in Dublin, and I never heard a practitioner make a statement in any degree calculated to leave behind an unfavourable impression of such similar proceedings. I may here give my own opinion, and that is, that if during this epidemic many of the ladies had been advised by their friends to go to Dublin, instead of elsewhere, there would not have been so much propping, touching, and falling out of gelatinous pessaries on door-steps. But from the useless sphere this branch of the science has advanced with rapid strides, and to gynæcologists of London, Dublin, and Edinburgh, practitioners and patients alike owe a deep debt of gratitude that never can be repaid.

Again, I say we should come up for re-examination, and show that we are not carried away by self-seeking while crossing the bar of our midwifery lives. Face to face before our former pilots, we ought to present ourselves somewhat after this fashion—"I am an open book, turn me over and over, read me through and through, and if you find a fault in me tell me of it, and I will score it through."

We can enter into this more fully with our midwifery teachers than with hospital clinical instructors, for with the former we run more in parallel lines, inasmuch as their teaching commenced at the initial stages; the latter, as a rule, with the last degenerative changes. We from the rank and file can impart much useful knowledge to the hospital physician and learned psychologist, but none to midwifery teachers, except, perhaps, a recital of small matters, such as—do not forget to have a number of four-penny nail-brushes, and, if you have no antiseptics, turpentine can be obtained in any village for cleansing purposes.

Before I close, let me say to our juniors, that it will add much pleasure to their lives, amid careers of singularities,

unreasonableness, and disappointments, if they cultivate patience, forbearance, and independence. Do not be disappointed at losing a family, a wretched fee, or a friendly nod from a fellow whom you would not pass as a first-class life for an insurance office. Only think what Corin said in *As You Like It*—"I am a true labourer, I earn what I eat, get what I wear, owe no man hate, envy no man's happiness, content with my harm, and the greatest of my pride is to see my ewes graze and my lambs suck." Indeed, we may all take a more refined and exalted lesson from the profound scholar and physician, the first great clinical teacher, Hippocrates. When his presence was commanded by King Artaxerxes, he sent a message to say that he had food, raiment, and habitation sufficient at home to enable him to pursue his calling, and to stand on his own individuality without having to wait on kings.

AN INTERESTING SOLID OVARIAN TUMOUR.

By ALFRED J. SMITH, M.B.;
Examiner in Midwifery, Royal University of Ireland;
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[Read in the Section of Obstetrics, December 20, 1895.]

I wish to bring especially under notice a short account of what was to me an extremely interesting case:—

Case.—C. G., aged twenty-five, unmarried, was sent up to my gynæcological wards in St. Vincent's Hospital, by Dr. Kelly of Eyrecourt, Galway. She enjoyed good health up to May, 1894. About this time she became troubled by frequent pains referred to the lower part of the abdomen, towards the left iliac fossa. She put off consulting a doctor until prostrated by a severe attack of influenza in the month of May last. At this time there was no swelling of the abdomen, at least nothing remarkable. However, Dr. Kelly made out an ovarian tumour, and sent her up to me for operation. I admitted her on the 1st October last; she looked very emaciated, with dark rings under her eyes and a typical facies uterina. She was unable to recline on the bed, so enormously distended was the abdomen; she had to be supported in the semi-erect position by an arrangement of pillows, an attempt to lie down on the bed caused intense dyspnœa. The patient stated that the enlargement of the abdomen was scarcely noticeable a couple of months before admission, that her belly suddenly became distended, and gradually grew larger and larger.

Abdominal palpation revealed an immense collection of free ascitic fluid, and no tumour could be made out. Vaginal examination threw no light on the case, owing to the great distension. The case was then transferred to the medical wards, under Dr. M'Hugh. I am indebted to Dr. M'Hugh for his report on the case while under his care. He says:—"Early in October last I saw C. G., in consultation with Dr. Alfred Smith. The patient exhibited extreme abdominal distension, with marked displacement of the heart upwards, and with dilatation of the superficial abdominal veins, including those extending upwards from the umbilicus. The

diagnosis was ascites from portal obstruction. The patient having been removed to my ward, I performed paracentesis on October 8th, removing fourteen pints of thin serous fluid. A tumour occupying the left inguinal and lumbar regions, was then distinctly palpable. The paracentesis was attended with a good deal of collapse, and with a little coffee-ground vomiting. During the week or ten days following the patient's strength gradually improved, and the amount of fluid in the abdomen increased. On October 20th she was transferred to Dr. Smith's care for removal of the tumour."

On examining the case under ether, an elastic tumour could be easily palpated; it floated freely about the abdominal cavity, its range of motion being very remarkable; it touched the ribs easily on either side; it ballotted quite sharply. The tumour was easily made out as growing from the left ovary, and the diagnosis of a long, pedunculated, solid ovarian tumour was made. I postponed its removal for a short time, hoping to be able to feed the patient up so as to increase her strength before operation. In this I was disappointed; the ascitic fluid increased so rapidly after the tapping that the distension became again enormous, and the ballottement of the tumour was so remarkably like that of a fœtal head that I was greatly impressed by the similarity.

I determined to operate without delay; the distension by the ascites was so great that the ether had to be administered to the patient sitting erect; her head was gradually lowered as the ascitic fluid escaped. The diagnosis was now established of a solid ovarian tumour—the abdominal incision was increased in size, and the tumour delivered in its entirety. (I append Dr. McWeeney's report of specimen.) The pedicle was thick, broad, and very vascular; it was ligatured with stout silk, using the interlocking chain suture. The large blood vessels were ligatured separately to give extra security, and the pedicle dropped. All went well with the patient until 36 hours after the operation. The nurse had administered 3 grs. of calomel, which caused nausea, and brought on a severe attack of vomiting; immediately the patient's condition so alarmed the nurse in charge, that I was hastily summoned about 10 o'clock, p.m. I found the temp. 96·6°, pulse, 120; patient restless; forehead bedewed with clammy sweat, and pupils much dilated. I at once recognised internal hæmorrhage. Surgeon M'Ardle joined me in consultation, and agreed with me as to the advisibility of immediate operation.

On re-opening the abdomen, fresh arterial blood welled up in

great quantity; there were few blood clots. I quickly secured the pedicle and looked at it; the ligatures had not slipped, and were apparently well secured, the portion of the stump above the ligature looked quite blanched, and it was difficult to make out where the hæmorrhage was coming from. However, on investigating further, it was found that an artery was pumping away immediately under the line of suture—right in the centre of the broad ligament and below the track of the silk sutures. The artery was well secured, and a silk ligature was applied by a Tait's needle, quite close to the uterus, taking in the proximal portion of the pedicle. As the patient was so blanched and pulseless, I poured in a large quantity of normal saline solution into the abdominal cavity. This caused an immediate and marked improvement in the patient; I closed the abdominal wound and did not drain. The subsequent history was quite uneventful; the recovery was slow; there has been no return of the ascites since the operation, now seven weeks ago.

Such, in short, is the history of this eventful case. The points which seem to me of special interest may be classified under the following heads:—

(1.) The cause of the ascites.

(2.) The cause of the hæmorrhage.

(3.) The value of immediate operation.

(4.) The prognosis.

Cause of ascites.—Although it is well known that ascites is caused by solid ovarian tumours, particularly when they are malignant, still I cannot find a record of such a small solid ovarian causing such an enormous distension as was present in this case. An interesting query, What was the source of the ascitic fluid? and How to explain the rapid filling after tapping? Is it to be accounted for solely by the purely mechanical causes of pressure and irritation?

(2.) *The cause of the hæmorrhage.*—Naturally the first impression is that the artery was left untied at the operation, and that the blood went on accumulating until the patient had lost so much that she collapsed. I felt quite satisfied at the time of the operation, as did Mr. M'Ardle, who assisted

me, that there was no bleeding when I closed the abdominal wound; a gauze sponge taken out of Douglas' pouch just before closing, was quite dry and not stained with fresh blood. I believe what may have occurred was that in perforating the broad ligament to introduce the suture an artery was damaged, perhaps partially torn through, and that the distress of vomiting completed the rupture.

That there was no hæmorrhage immediately following the operation is, I think, established by the following facts:—

(*a*) The length of time from operation until collapse supervened.

(*b*) The patient's condition was remarkably satisfactory up to the attack of vomiting.

(*c*) The sudden state of collapse evidenced by the acceleration of the pulse-rate and fall of temperature.

(*d*) The fresh appearance of the blood and the absence of tarry-looking blood clots.

(3.) *The value of immediate operation.*—This case demonstrates the value of immediate operation in cases of internal hæmorrhage following an abdominal section, and illustrates that secondary operations do not materially influence the result.

(4.) *The prognosis.*—It is a fact that should be emphasised, that convalescence is very tardy after ovariotomy for sarcoma. Almost all the authorities are agreed that relapse is greatly to be dreaded. Thornton records ten cases operated on by him; three died from the operation; of the seven which recovered one remained in good health, and had a child two years later—one died a few months after operation, and another eighteen months after—both from recurrence; and four died within a year of the operation from dissemination of the growth. The most hopeful view, however, is C. Braun's, who, in the *Centralb. fur Gyn.*, 1889, No. 36, reports a well-authenticated case of ovarian sarcoma in which a cure had been maintained without relapse for eleven years.

Pathological Report on Specimen by PROF. MCWEENEY :—

"This is a large solid tumour, which was at first nearly globular, and is now somewhat oval and flattened by its own weight.

"*Size.*—It is nearly as large as an adult head; dimensions: circumference of long diameter, 58 cm.; of short, 53½ cm.; diameter of sectional area, 19 cm.

"*The colour* is now almost pure white; the specimen is in alcohol.

"*Consistency* solid and firm throughout, but not quite as hard as an ordinary fibro-myoma.

"*Naked-eye anatomy.*—Outer surface everywhere smooth, consisting of a fibrous capsule covered with peritoneum; at one place is a large triangular ragged spot corresponding to the pedicle, and showing the separation of the layers of the broad ligament. From this spot diverge numerous extremely large and thin-walled vessels, apparently veins, which come from various parts and converge to form a sort of venous sinus at the point where the pedicle was divided. Close by was a fold of broad ligament attached to the tumour, containing 6 cm. of the Fallopian tube, including its abdominal end. The canal was pervious to a fine probe and apparently healthy. On section the tumour has a banded appearance, but the bands are not so hard and shining as in fibro-myoma. The intervals between the bands are yellow and mottled. There are some softened and degenerated patches, but these are neither so large nor so numerous as we generally find in tumours of this size—a fact which is amply accounted for by the high vascularity of the growth.

"*Microscopic anatomy.*—The tumour consists of bundles of spindle-shaped cells, with very much elongated nuclei approaching and perhaps identical with unstriped muscle. Still the nuclei are not typically rod-shaped, as are those of unstriped muscle. The bundles are very close-packed, and interlace in various directions. From the extreme richness in cellular elements, and their not complete identity with fully-formed adult unstriped muscle, I think the tumour should be classed as a myo-sarcoma of the ovary. Such tumours are very rare; according to Bland-Sutton, they are not so liable to secondary changes which affect similar tumours of the uterus or the broad ligament."

A CASE OF OVARIOTOMY.

By R. J. KINKEAD, M.D.;
Physician and Gynæcologist to the Galway Hospital;
Professor, Queen's College, Galway.

[Read in the Section of Obstetrics, December 20, 1895.]

Mrs. O'N., aged sixty, the mother of eight children, the youngest of whom is eighteen years of age, was admitted under my care to the Galway Hospital on 25th September, 1895.

I had been brought out some nine miles to her house on 8th April, as "she was bad with the dropsy and needed relief." She was in bed, half dressed, as is the custom of the country, in a small ill-lighted room, containing two wooden bedsteads with high sides, besides a big chest and other furniture; so that examination of the patient was difficult.

She was propped up with pillows; respiration shallow and hurried; pulse rapid; face anxious; abdomen distended and tense, dull on percussion, except in upper portion; fluctuation distinct; per vaginam, no tumours could be felt; Douglas's pouch empty.

The surface of abdomen was smooth; the swelling reached a couple of fingers' breadth above umbilicus. I diagnosticated an ovarian cyst, and strongly urged her removal at once to hospital. Both patient and relatives refused to entertain the idea, and I did not see her again until 26th September, the day after her admission to hospital.

Her abdomen had increased in size, and now was enormously distended. She could not lie down. Her pulse was rapid and weak, though regular; breathing very shallow and hurried; face pinched and haggard—she had lost flesh, though not emaciated; fluctuation distinct all over the abdomen, the surface of which was smooth and tense, with large veins ramifying over it; towards left inguinal fossa nodular masses were felt.

The entire of the anterior surface of the abdomen was dull on percussion, except close under margin of ribs; the dulness did not vary its position on change of posture; the right flank was also dull, but a resonant note could be obtained on left flank, and back

towards lumbar region, when patient was on right side; heart and respiratory sounds were normal; and, although scanty, the urine was free from albumen and sugar.

Assisted by my colleague, Professor Brereton, and by Dr. M. Lydon, house surgeon, who administered the anæsthetic, I operated on 13th October.

A purgative had been administered on the 11th, followed by an enema on morning of 12th, on which day the abdomen was well washed with soft soap and hot water.

On the morning of the operation the pubes were shaved, and the entire abdomen and thighs were washed. Immediately after the washing two or three folds of lint, soaked in a warm solution of carbolic acid, 1 in 40, were laid over the abdomen.

Chloroform was used at first, and when the patient came fully under its effects ether was substituted, and anæsthesia maintained by it to the end of the operation.

A catheter having been passed and a couple of ounces of urine drawn off, I removed the carbolised lint and opened the abdomen in the mesial line, bringing tumour into view, its surface glistening and of a green colour.

Adhesions had formed extensively between the cyst's surface and the abdominal wall, which, however, were easily broken down. Above, the omentum was firmly adherent; and from the top of the tumour, and towards the left, a strong band of adhesion, some two inches wide and three long, could be felt passing upwards to the parietes.

Having tapped the cyst with a Spencer Wells' trocar, and partially emptied it, I was obliged to extend the incision, originally seven inches long, by at least two inches, in order to reach and deal with these adhesions. It was impossible to separate them from the tumour: the omental one was double ligatured, and cut off between the ligatures; that from the abdominal wall, having been carefully examined, and found free from visible vessels, was cut off close to the cyst: there was no bleeding from the cut ends. Fortunately, there were no adhesions to the intestines or pelvis; and, on being emptied, the cyst, with a solid base, the size of a dinner plate, composed of numerous smaller cysts, was easily drawn out through the wound.

The appearance of the pedicle, uterus, and vagina was at first puzzling; the greater portion of the tumour was green in colour, but towards the pedicle it became pink, and the colour of its

lower portion was identical with that of the uterus and vagina; the pedicle was twisted and very short, so that it did not at first sight appear to be a pedicle, but looked as if the tumour was merely constricted; that the uterus, projecting sideways through the lower angle of the wound, was a portion of it, swelling out below the constriction; and that the vagina was the pedicle—a twist in the vagina making the appearance more real.

The tumour had to be twice turned completely round, from right to left, to untwist the pedicle; it was fleshy and very short, not more than ¼ inch, so that the tumour appeared to spring directly from the left uterine corner. So short was the pedicle that, after transfixing and tying it as close as possible to the uterus, I was obliged to dissect a portion off the outer surface of the tumour to form a stump to prevent the ligature slipping. The hæmorrhage was trifling; very little blood escaped into the abdominal cavity.

The abdominal wound was closed by interrupted sutures of silkworm gut, passed from within outwards through entire thickness of the walls, and placed half an inch apart; the wound was dusted with iodoform and boracic acid, 1 to 7, and dressed with gauze and boracic wool.

From commencement of incision until the patient was placed in bed occupied exactly an hour.

She bore the operation well, and there was no vomiting either during or after it. For first twelve hours she got only teaspoonfuls of hot water at frequent intervals; during the night half a pint of beef-tea, and the next day milk and beef-tea alternately.

The catheter was used every six hours for two days, and bowels were moved by enema on fourth; half the sutures were removed on seventh day, and the other half on the eighth.

There was very slight tympanitic distension of the abdomen; no vomiting all through; but trouble was caused by the tendency to formation of fæculent masses in the intestines, which could be felt in ascending, transverse, and descending colon, necessitating the administration of aperients in addition to enemata.

The wound healed by the first intention. The patient sat up in bed on the tenth, and was allowed up on the eighteenth day, after which the bowels operated regularly.

Sixteen quarts of a dark brown fluid were drawn off by the trochar.

I annex a report on the tumour, and exhibit photographs

kindly supplied me by Dr. R. Allen, M.B., and Travelling Scholar, R.U.I.:—

"The tumour is a unilocular cyst, and was attached by means of a pedicle which is in connection with the remains of the ovary. The pedicle was made up of the left Fallopian tube, the ovarian ligament, and a portion of the broad ligament.

"The cyst contained 640 ozs. of fluid. This fluid was of a watery consistency and dark brown colour. There was also found in it a large amount of dark blood clot and a thick viscid mucoid substance. From the inner wall several smaller cysts spring, three of which are larger than a hen's egg, the largest being multilocular, somewhat cubical in shape, and with sides measuring 5 inches. These cysts contain a thick, glairy fluid, dark green in colour, with which they were tensely distended. Their walls are thin and translucent, and in the unopened state they presented a dark green shining appearance, due to the character of their contents.

"The tumour weighs when empty 56 ozs.

"The cyst wall is thickest towards its posterior portion. On the anterior surface the remains of omental and peritoneal adhesions are evident; and in the substance of the wall in this region are a number of blood extravasations. Large blood-vessels, arteries, and veins are found, running from its pedicular attachment, and ramifying over its surface. On the inner surface the whole of the anterior portion of the cyst wall was covered with a firmly adherent layer of dark brown blood clot. A large amount of discoloured blood clot was also adherent to the surface of the largest daughter cyst.

"The Fallopian tube is not dilated or elongated, but the ostium and canal are open; the walls are thickened, the thickening being most marked towards the fimbriated extremity. A trace of the ovary can be detected: it is spread out and shrivelled, and on its surface there is a small translucent cyst ½ inch in diameter.

"*Microscopical characters*:—The wall consists of fibrous tissue; in some parts it can be split up into three or more layers. Flattened and elongated connective tissue cells are found in small numbers, having the usual distribution between the fibres. It is covered externally with flattened nucleated cells. Internally, cells are found lining it in some parts: a few of these present columnar characteristics, and the majority are of a regular flattened variety: however, the greater portion of the internal surface is devoid of

cellular elements. The structural elements to be detected in the fluid are round granular cells. The most of these are non-nucleated; but in several a distinct nucleus is present. Columnar nucleated cells are present in small numbers, and a large amount of small granules. The small cysts possess a somewhat similar structure: their walls are fibrous, and lining their inner surfaces is a layer of cubical cells. The viscid fluid contained in these exhibits similar structural elements. On examination of the remains of the ovary, Graafian follicles are recognised; and the small cyst above mentioned as connected with this part contains a well-marked membrana granulosa lining its interior: no ovum, however, can be detected. This cyst is of interest, showing clearly a stage in the cyst formation, and giving an indication as to the origin of the large tumour, which may be described as a unilocular oöphoritic cyst.'

NOTE ON OVARIOTOMY.

By HENRY GRAY CROLY,
Surgeon, City of Dublin Hospital.

[Read in the Section of Obstetrics, December 20, 1895.]

MR. HENRY GRAY CROLY exhibited a large multilocular ovarian tumour which he removed from a girl, aged sixteen years, in the City of Dublin Hospital, on July 2nd, 1895. The tumour commenced to grow about nine months before her admission to hospital, and was first observed on the right side. The abdomen was very large, and numerous veins ramified over the tumour. The fluctuation was very distinct high in the abdomen; less so towards the pelvis, where solid masses were felt. Menstruation was irregular of late, and the "ovarian face" was very marked. The girl lost flesh considerably. The measurements were 38 inches in circumference at level of umbilicus, 30½ inches at ensiform cartilage, 8½ inches from umbilicus to right anterior superior spine. 8¾ inches from ensiform cartilage to left anterior superior spine. The usual incision was made, and Mr. Croly found it necessary to prolong the incision upwards and to the left of the umbilicus. A very large and distended vein in the broad ligament lay across the upper part of the cyst. The vein was secured by double sterilised silk ligature, and divided between them. Two gallons of gelatinous, greenish fluid were drawn off from the large cyst which Mr. Croly then opened, when several more solid tumours were found.

Mr. Croly found some difficulty in removing the tumour from the pelvis. This was caused by the locking of the smaller cysts in the pelvis. These cysts were opened with a scalpel, and a boiled starch substance escaped. The

pedicle was tied in the usual way, and no drainage was adopted. There was no hæmorrhage. The girl was fed "per rectum" for several days, and made a rapid recovery and got fat. She returned to the country, and is in perfect health.

MISPLACED AND ROTATED SPLEEN WHICH SIMULATED AN OVARIAN TUMOUR.

By HENRY GRAY CROLY,
Surgeon City of Dublin Hospital.

[Read in the Section of Obstetrics, December 20, 1895.]

THIS spleen was removed from a married woman (aged forty years) in the City of Dublin Hospital, on the 7th inst. The woman had several children; but never noticed any abnormal tumour until last summer. The swelling commenced at the left side, and gradually extended towards the right; was not painful. She lost flesh and her features changed. She thought at first she was pregnant. The doctor who attended her previously never observed any abdominal tumour until he was consulted after her last confinement. She aborted shortly after her admission to hospital. On palpation the tumour was firm, and gave the sensation of fluctuation, and, though apparently larger on the left side, crossed well over to the right, and could not be moved upwards or downwards. There was no history of ague, and the woman was never out of her native place. The usual vaginal and uterine examinations were made by the skilled gynæcologist to the hospital and the distinguished Master of the Rotunda Hospital.

When the patient was first admitted to hospital no decided opinion was expressed beyond that it was an "abdominal tumour," and no clinique was given, but the case was carefully palpated, and attention paid to improving the lowered condition of the patient's health. She was well fed, and allowed into fresh air, and walked as much as she felt inclined or equal to. After the final examination of the tumour by the gynæcologists the unanimous opinion expressed was the

belief of its being ovarian. The question of the difficulty of diagnosis of abdominal tumours of all sorts is well known, and this proved no exception, and, though doubts were at first expressed on the case, all at the final examination believed the case to be ovarian.

I performed laparotomy, and came down on a large, solid, fleshy mass, with purpuric mottling on the surface. The small intestines were adherent to and fixed by the tumour, but were freed by the fingers. There was no hæmorrhage; as the mass was firmly fixed and could not be raised, the pedicle was sought for in the usual position, but not found; the wound was then extended above and to left of the umbilicus, where a funis-like pedicle was discovered, feeling like a bar of iron. This was surrounded by omentum. The hand was now passed along the pedicle and the spleen space was empty. The pedicle was secured by a stout double silk ligature. On section, the mouths of vessels were seen as in an "Esmarched limb." There was no blood lost; no vessel required ligation, torsion or clip. The peritoneal cavity and pouches were sponged. The abdomen was not closed until all risk of bleeding points was made certain. The patient bore the anæsthetic and operation, which was rapidly done, well, and went on most favourably for days, when vomiting set in and symptoms of collapse, and she succumbed. No *post-mortem* examination was obtained.

The following discussion took place on the three papers:—

Dr. E. Tweedy remarked that Dr. Smith, in his paper, said nothing about having examined the second ovary. Sarcomatous tumours occurred, as a rule, in both ovaries: here sarcoma occurred in only one. He was not quite clear that Dr. Smith's case was one of ordinary sarcoma. Referring to Mr. Croly's case, he said that unless the pedicle could be felt no certain—at least only an approximate—diagnosis could be made.

The President said Dr. Smith's paper put him in mind of

a case that came under his care fifteen years ago. She had what he diagnosticated to be an ovarian tumour, with ascites. After opening the abdomen it became so wedged in the brim of the pelvis that he could not well get it out, and it was so soft that it broke down. One of his assistants had to force the tumour up from the vagina before he could remove it. It proved to be a sarcoma. She is now married, and her only cause of regret is that she has no children. He asked Mr. Knowsley Thornton his experience, and he was of opinion that, without exception, in every case in which the disease had occurred the patient died within twelve months. His case was important, inasmuch as it proved that sarcoma of the ovary was not necessarily fatal, and that only one ovary may be affected. In the absence of malignant disease there must be some constitutional cause of the ascites. When he (the President) was a student, some twenty years ago, Dr. Stokes laid it down as a law that if ascites was present the disease was most likely malignant, and he (the President) thought that law held good at the present day.

Dr. Glenn said that in differentiating between an ovarian and splenic tumours, the points to be relied on were—(1) the presence of a notch; (2) the consistency of the tumour; (3) and its position.

Dr. Macan could not see why Dr. Smith did not make his diagnosis himself instead of handing the case over to his medical colleague. He thought another diagnosis might have been made, and that there was another cause for the ascites—that was, tubercle. He had opened an abdomen himself and found not a trace of fluid, where there was no doubt that the disease was carcinomatous. He congratulated Dr. Smith on his paper. He could not gather the evidence on which it was stated that the spleen was likely to suppurate in Mr. Croly's case. His diagnosis of the case was that the woman had been going about with a twisted spleen for many months, and that enlargement was due to the twist.

Dr. Lane, speaking of the tumour, said Mr. Croly referred to pregnancy. He asked the patient was she pregnant and she said no. He was under the impression that it was a fibro-cystic tumour. On examination, per vaginam, he found the cervix softer and more congested than is the case in pregnancy. The uterus was also more increased in size than it would be in pregnancy. To his mind it had a very elastic and fluctuating feel. When the abdomen was opened the tumour had gone up to the diaphragm, and it seemed that the pedicle was going down into the pelvis, and that it was not going upwards towards the left side.

Mr. M'Ardle congratulated Dr. Smith. He mentioned three cases of sudden hæmorrhage. One was from a friable pedicle. In all three cases referred to the rapidity with which the pulse returned was well-marked after injection of saline solution. At the time they were securing the bleeding points the patient was blanched, and large beads of perspiration stood on the forehead. The shock that was often spoken of in connection with these operations meant often that inflammation had spread along the pelvic veins. He was acquainted with two cases where death was attributed to shock; but the fact was, in each case, a cast of the inferior vena cava was found in the right side of the heart. He believed the use of the clamp forceps set up phlebitis, and was a cause of the high mortality in those operations. He pointed out the advantage of digital pressure practised in a manner shown. In the diagnosis of abdominal tumours he laid stress on elevating the patient in the Trendelenberg position. Unless the tumour was adherent to the rectum it would move upwards towards the diaphragm. He thought thrombosis of the splenic vein accounted for the hard cord.

Dr. Winifred Dickson mentioned a case in which there was ascites, yet the case was an ordinary cyst. Dr. Dickson mentioned the fact, as a good deal had been said regarding ascites accompanying malignancy.

Dr. Horne said when they were examining the case they were struck by the amount of ascites present. With regard to the bleeding occurring 36 hours after the operation, they should remember that Dr. Smith pointed out that the pedicle was very broad—it measured 5 inches in breadth. He connected the bleeding with the vomiting. He also made some remarks on the histological character of the tumour.

Dr. Smith, in reply to the remarks made on his paper, said he could give no explanation of the ascites. The fact that Dr. Atthill's case is still living gave him hope. He did not clearly understand what Dr. Atthill meant by saying the cause of the ascites in his (Dr. Smith's) case was constitutional. He commented on Dr. Macan's view as to the tubercular nature of the disease.

Mr. Croly replied to the observations of Dr. Macan and other members. He could not agree with Mr. M'Ardle as to the value of turning the patient upside down, seeing that the tumour was adherent in all directions.

CASE OF SO-CALLED ELEPHANTIASIS.

By FREDERIC W. KIDD,

Master, Coombe Hospital.

[Read in the Section of Obstetrics, January 31, 1896.]

THE specimen which I submit this evening is one, I think, of sufficient rarity to justify me in bringing it before the members of this section of the Academy. The history of the patient is briefly as follows :—

C. O'H., aged thirty-three (?), was, through the kindness of Dr. Blake, of Ravensdale, Co. Louth, sent to the Coombe Hospital on the 27th of November, 1895. She complained of "a lump growing on her private part." She was quite healthy looking, her face bronzed by constant exposure to the weather. She had little difficulty in walking, but said that the rapid increase in size of the growth had prevented her from sitting down. She stated that some thirteen years ago or more she "got a fall," and that a lump came on the right labium. This did not increase very much in size until

about eighteen months ago, when it increased rapidly, until it had reached its present proportions. She had never sought medical advice until she saw Dr. Blake on the 23rd of November. She seems in excellent health; menses occur at regular monthly intervals lasting three days, normal in quantity, and unaccompanied by pain. She has no leucorrhœa. On examination, the tumour (of which I now submit an excellent photograph, kindly taken by Dr. J. Alfred Scott) exhibits the following features.

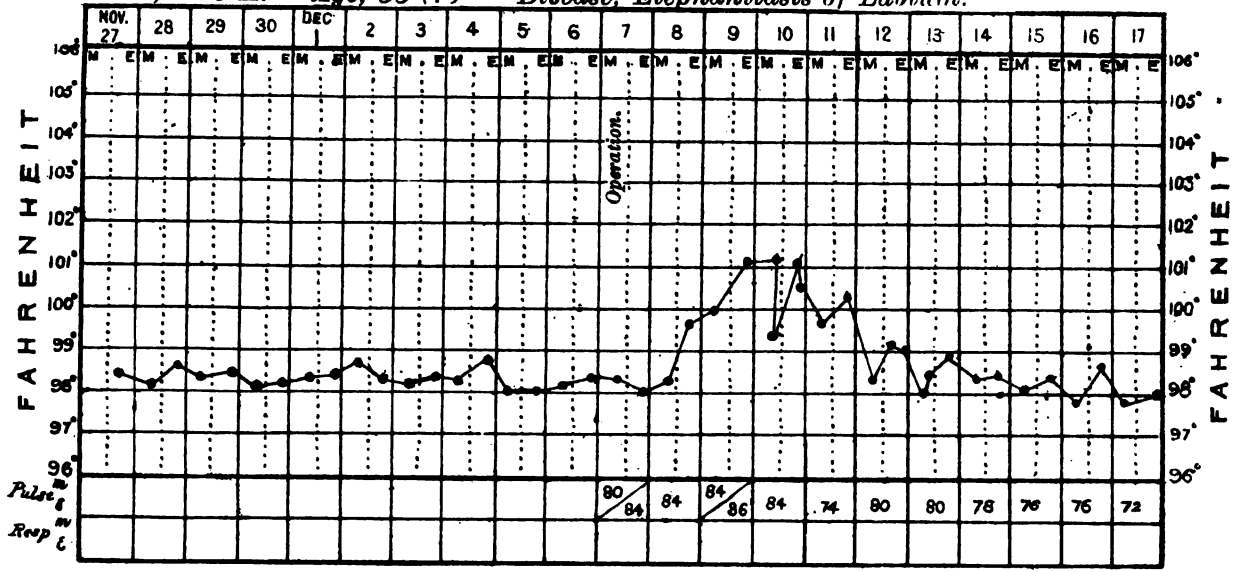
Naked-eye Appearances.—"The tumour, which weighed 4 lbs. 5 oz., is whitish in colour, covered close to the pedicle with rather dark-coloured skin, from which grow a few long hairs, whilst elsewhere it is covered with epithelium which has the characters of mucous membrane. It is lobulated and very irregular in shape, with numerous papilla-like excrescences, so as to resemble a gigantic wart. The consistency is rather soft. Between the larger lobes are curious sinuous cavities, some of which are actual passages or canals running right through the mass, from near the pedicle at one end to the free convexity on the other. Part of the skin covering the base of the tumour is covered with innumerably small pedunculated protuberances like fungiform papillæ."

For the foregoing description, as well as for the microscopical report, I am indebted to Dr. McWeeney, who kindly made the necessary examination. There was a good deal of semi-purulent discharge oozing from these "sinuous canals or cavities," and it took repeated hot baths, careful washing and constant dressing with lint, wrung out of carbolic solution, to get the tumour in a satisfactory condition for operation. One of the photographs [exhibited]—that taken at the longer focus and lettered B—represents the patient sitting on the edge of a chair with the thighs widely separated, and very clearly shows how the weight of this tumour so dragged on the skin and subcutaneous tissues in front of the symphysis pubis so as to form a kind of suspensory ligament. The other photograph—taken at a nearer focus and lettered A—represents the patient lying on a gynæcological chair with the legs supported at the knees in crutch supports, and the thighs widely abducted. It demonstrates very clearly how the growth would interfere with the comfort of the patient in the sitting posture, and gives a very excellent general idea of the warty appearance of the whole growth. The patient had no trouble or difficulty in micturition.

The Operation (7th Dec., 1895).—Patient having had hot baths daily, and having had the tumour enveloped in cloths wrung out of

carbolic solution, was prepared in the usual way for operation. About 12 30 she was put under influence of ether by Dr. Allen. Assisted by Dr. Heuston and Drs. Stevens and E. Winifred Dickson, a preliminary and careful examination was made of the relation of the parts. The vagina was found, after some difficulty, posterior and to the left side of the tumour. On digital examination the uterus could be felt, normal in size and position; the left labium, major and minor, could be seen, but there was no evidence of the presence of any hymen. There was a noticeable absence of growth of hair either where the mons veneris should be about the left side of vagina or on the tumour itself. On passing the finger into the vagina itself an opening which admitted the top of the finger was felt in the situation, where one would expect to find the meatus urinarius. On passing a cathether into this, it passed up several inches, and the tip of the catheter could be felt high up anterior to the symphysis pubis. There was evidently a large cavity or cyst here in direct communication with the vagina. It was only with the greatest difficulty that the slight dimple of the meatus could be felt between the opening of this cyst into the vagina and the opening of the vagina proper. First of all a linear incision was made across the upper portion of the growth, marking how much tissue would have to be removed; then the dissection of the growth was proceeded with, bleeding points of any consequence being "clipped" or tied as they occurred. When the mass was removed it was found that a great portion of the large cyst into which the cathether was passed had been left behind, and had to be dissected out for a considerable distance in front of the pubis. Its attachments were very dense; they cut with a grating sound, and quickly blunted the edge of the knife. Finally, it was all removed, and all bleeding points were tied—that portion corresponding to where the clitoris would naturally have been causing the most trouble, and finally requiring to be transfixed by needle, threaded with ligature, to prevent slipping. The mucous membrane had to be stitched to the skin at the lowest angle of the wound. A drainage tube was inserted the whole length of the wound (about 8 inches). The wound was stitched with Reverdin's needle and silk-worm gut, a dressing of iodoform gauze, covered by a pad of sterilised Gamgee tissue, and a T-bandage applied, and patient put to bed. Although there never at any time was any excessive loss of blood, yet a good deal was lost by oozing during the operation; the latter lasted about an hour and a half. Patient got rather collapsed towards

Name, C. O'H. *Age, 33 (?).* *Disease, Elephantiasis of Labium.*



the end, and had to get an enema of brandy and beef tea, but she rapidly reacted when put to bed. The tumour and pieces, in the recent state, weighed 5½ lbs. There was a small cyst-like mass immediately anterior to the meatus which was not removed, as the patient exhibited symptoms of some collapse towards the end of the operation, and it would have necessitated a further slitting up of the vagina to remove it. In case this exhibited any tendency to increase in size I think it should be removed.

The following is Dr. McWeeney's report of the microscopical examination:—

Microscopical Examination.—"The tumour consists of a somewhat œdematous fibrous tissue of loose texture, containing so many lymph clefts that in places one might almost term it a lymph-angioma. There are also very numerous newly-formed blood vessels; around the lymph clefts are many collections of lymphoid and epitheloid cells; sections of the papilla-like protuberances show essentially the same appearance. The surface is covered with stratified epithelium, which, from its slightly developed horny layer and absence of hair follicles, may be said to constitute a mucous membrane. I should be inclined to class this specimen provisionally as '*molluscum fibrosum.*'"

Subsequent History and Treatment.—The dressings were removed three times a day, as there was a very profuse watery sanious discharge at first, which quickly soaked the dressings. Patient complained of much thirst after operation for two or three days, and tongue was rather dry and slightly coated. On the third day after the operation this latter symptom was well marked; the patient complained of headache and had not slept well. On proceeding to dress the wound, it was found that the upper angle of the wound had pus oozing from it; the uppermost stitch had ulcerated through and was removed; the stitch next to it was removed to relieve tension; the suppurating cavity was carefully syringed out with boracic acid solution and plugged with iodoform gauze. This cavity did not seem to be connected with the cavity in which the drainage tube lay. The lower part of the wound was very much swollen and œdematous, and of a reddish hue. All this time the patient had no trouble in passing water. At the second dressing on this day, and subsequently, the drainage tube was taken out and washed, then re-inserted until the 13th inst., when the tube could not be re-introduced. With the formation of pus the tem-

perature went up, but never higher than 101·2°. I here submit the temperature chart up to the 17th December.

It was found that the needle hole of the uppermost stitch that had been removed communicated with the cavity in which the tube lay, so that the whole length of the wound could be syringed through. On the sixth day all the silk-worm gut sutures (seventeen in number) were removed, except three that were at the extreme left and lower part of the incision, and which were removed four days later. During all this time the patient passed water herself freely, and the bowels were well moved on the third day after the operation. She complained of thirst for two or three days after the operation, and her tongue became dry and somewhat furred; this was relieved by small doses of dilute hydrochloric acid. Nine days after the operation she was able to lie on her side. Some of the stitches at the lower angle of the incision, where skin and mucous membrane were stitched, did not hold, nor is this to be wondered at; but the tissues healed with healthy granulations. Union elsewhere was very good, except the two stiches at top, which was all the more surprising considering the low form of vitality which is usual in skin covering such growths, and considering the great amount of œdema which took place afterwards. Some slight œdema still exists at lower part of incision, and at lowest portion of the buttock on the right side.

Nomenclature.—As regards the nomenclature of this growth, the term "elephantiasis" was only used in a broad sense as including several forms of tumour having a somewhat similar appearance, but having essential differences histologically. No one would for one moment suggest that it was a true case of elephantiasis arabum, caused by frequently repeated attacks of erysipelatous inflammation, affecting skin, subcutaneous tissue, veins, lymphatics and lymphatic glands, tending to hypertrophic changes that sometimes reach such development as to render them hideous deformities.

No one would compare such a growth as this to the spurious elephantiasis, or elephantiasis lymphangiectodes, caused, according to Dr. Lewis, by the blocking up of

lymphatic channels, by the pressure of filariæ sanguinis hominis—a parasite supposed to infect dogs in Eastern countries. This form differs in its history from that just described, in that there seldom are repeated inflammatory attacks; its advent and growth are gradual and insidious until a very considerable growth has been attained. Vesicles, which are dilated lymph spaces, make their appearance, and may exude from time to time a more or less profuse quantity of coagulable lymph. Moreover this form only occurs in Eastern countries. In this case there is no history whatever of recurrent inflammatory attacks. The cause is attributed, whether rightly or wrongly, to an injury received from a fall, so that it seems most to conform to the description of the growth known as molluscum fibrosum, or molluscum pendulum of Wilan. The patient went home fat and well, and up to the time of going to press there has been no return of the growth.

The President had only met with one case analogous to Dr. Kidd's, and that was in his private practice many years ago. The excessive growth of tissue, however, had not reached one-third of the dimensions seen in Dr. Kidd's case, still the disease caused great distress. There was marked pruritus. One of the labia was enormously enlarged. The tumour sprang from one side; the other side being healthy.

Dr. Jellett had the opportunity of observing a case in many respects similar to Dr. Kidd's. One labium was three times the size of the opposite labium. The skin was hypertrophied and pigmented, and over its surface a few hairs were to be seen scattered here and there. On section of the tumour he found traversing bands white in colour and homogeneous in appearance. They were not unlike cartilage. He had some reason to think that the capsule of the tumour consisted of perineurium.

Dr. Kidd said that it was usual to refer cases like his to repeated attacks of an erysipelatous character, or to obstruction of the lymphatic channels. He could not trace a pathological connection in the present case between the tumour and the nerves. He could not agree with Dr. Jellett that the tumour originated in nervous tissue.

ECLAMPSIA.

By E. HASTINGS TWEEDY,
Fellow and Examiner, Royal College of Physicians;
Late Assistant Master, Rotunda Hospital.

[Read in the Section of Obstetrics, January 31, 1896.]

In bringing the subject of eclampsia under notice, it is not my intention to treat of it in all its branches, or occupy the valuable time of the Section by recapitulating facts concerning which the leading text-books of to-day fully inform us. Neither shall I enumerate the many theories advanced as to its causation, but shall rather endeavour to point out what I conceive to be the sound principles of treatment, based on the present facts known in connection with this complaint.

Almost all are agreed that the disease arises from an accumulation in the system of a toxin poison—a normal resultant of tissue waste—which the kidneys, acting in an abnormal manner, fail to eliminate.

As to the *modus operandi* of this poison I shall say nothing, believing that such speculative inquiry is outside the province of practical obstetricians, and I look forward to the day when our text-books shall cease to be padded with fairy tales of physiology.

Whatever we may call this toxin, I believe it to be similar to that which gives rise to the convulsions known as uræmic, and the arguments adduced to prove the dissimilarity of the fits are for the most part worthless. It is urged, in the first place, that the convulsive attacks differ in character, but I have not been able by personal observation to detect any difference; and certainly those mentioned by authorities,

such as a different range of temperature, &c., are entirely untrue.

Again, it is said that eclampsia is not often associated with chronic forms of kidney disease, but with an acute variety that has earned for itself the name of the "kidney of pregnancy;" while, on the other hand, uræmic convulsions are equally as rare in the acute variety of Bright's disease. Before answering these objections, let me for a moment refer to this "kidney of pregnancy," for I fail totally to see on what grounds it came specially to be described as such. So far as I can discover, it differs in no way from the ordinary degeneration kidney seen under many other circumstances.

In pregnancy it is accompanied by a certain amount of parenchymatous nephritis, and arises in most instances as a consequence of that disease. Neither is it by any means constantly seen as a kidney lesion in eclampsia.

To give an answer to the question why acute nephritis is so often associated with eclampsia, we must remember that the centres of nervous control are in a highly unstable condition throughout pregnancy, their functions being often suspended by comparatively trivial causes. It is therefore to be supposed that a smaller dose of poison will exert a graver influence in the pregnant than in the non-pregnant condition. In addition to this there is an absolute increase of toxin manufactured during pregnancy, for not only are metabolic processes more active in the woman herself, but likewise her blood receives the effete material formed during the growth of the fœtus.

This latter factor appears to largely account for the onset of eclampsia, for we find that not only do multiple births predispose to the attacks, but that the death of the fœtus enormously improves the prognosis of the condition.

Again, the rarity of eclampsia in connection with chronic Bright's disease is no cause for wonder, for this latter disease

is rare in pregnancy. It leads, as a rule, to endometritis, which in its turn leads to sterility or early abortion; while of those cases of pregnancy which persist in spite of chronic Bright's disease, as large a number (if not a larger) develop convulsions than would do so in the unimpregnated condition.

There is apparently a strong argument urged against the theory which supposes a urinary toxin in connection with this disease—namely, that eclampsia sometimes occurs without any apparent kidney disease, and no diminution in the quantity of urine secreted. In connection with this point, the observations of Dr. Ethridge, published in the June number of the *American Gynæcological Journal*, are very valuable.

He finds that many women, under certain depressing influences, secrete for long periods at a time an enormously diminished quantity of urinary solids, the kidneys at the same time being in an apparently normal condition, and that these patients exhibit symptoms similar to those known as premonitory in eclampsia.

Thus we see it is not necessary to pre-suppose disease of the kidneys when accounting for a self-intoxication sufficiently potent to excite fits under conditions favourable for their onset.

It is impossible to doubt that the kidneys are the chief organs concerned in the elimination of the uræmic poison. That the skin and intestinal mucous membrane get rid of it to any appreciable amount is greatly to be doubted. Nature appears undesirous of relegating the functions of the kidneys to any other set of organs, and for my part I do not much regret that she has withheld from us the luxury of uriuating through our sweat glands. Why do those who believe that the skin can act as a third kidney not settle the question, by injecting the sweat of a uræmic patient into some small animal, and thus contrast its poisonous effect with that

of normal perspiration? Experiments such as this have already been undertaken to test the toxidity of the urine at different periods of the eclamptic attack, with the result that it has now been fully shown that the urine is poisonous to its highest degree during the convalescing period of the disease. I am told of patients who have been seen covered by a fine flour-like deposit after a severe sweating; this phenomenon must be very rare, and possibly may be due, when it does occur, to a diseased condition of the sweat glands.

In dealing with the treatment of eclampsia in the absence of any specific remedy, our efforts should be directed to counteract as many of the gravest conditions tending towards death as possible. Thus the introduction of cleanliness in midwifery has enormously lessened the death-rate in eclampsia by removing sepsis from the list of those conditions. If we except this latter as a cause of death, our present statistics bear very unfavourable comparison with those of former years, and I think the proofs are convincing that we are not at present moving along rational paths.

Chloroform may be said to be now in general use in the treatment of eclampsia, and to this the administration of chloral and of bromide of potassium is generally added. All these drugs act as powerfully depressing agents to the heart, and tend to kill in precisely a similar manner to that of eclamptic poison; for all are agreed that heart failure, followed by œdema of the lungs, is amongst the most constant precursors of death. Furthermore, chloroform as usually recommended is applied only on the occurrence of the first signs of an approaching fit. Given in such a manner, it has but slight effect in lessening the severity of the attack; and by limiting the amount of oxygen that would otherwise pass into the lungs, it in some cases actually increases the severity of the convulsions. Much reliance, also, is placed on getting the skin to act vigorously, the

supposition being that toxins are thus eliminated. It cannot be claimed for this plan that it is at all times easy in its accomplishment, very certain in its good effects, or even safe in its application—always some depression, and occasionally alarming collapse, follow from it.

I do not believe that a more fatal drug could be found for uræmic patients than pilocarpin. It is a heart sedative of the first order, and it has the additional power of promoting free salivation and bronchial secretion—very dangerous complications of the eclamptic state. But in the face of so much experience to the contrary, I am slow to think that at least a temporary improvement in the patient's condition does not follow this treatment, and an explanation of its action has occurred to me.

Convulsions result *not* from the presence of toxins in the blood, but rather from their deposition in the more solid structures of the body, especially in the nerve centres. Now, to deplete the blood by sweating would be to create a current proceeding from these structures into the blood to make good its deficiency in water; in this way the convulsive centres will get rid of some of the poison, the blood at the same time becoming richer in toxins. Whether you accept this explanation or not, I do not think that severe sweating is a justifiable proceeding, and hope to see it soon more generally condemned. Efforts also at giving drugs, stimulants, or nourishment by mouth, must be emphatically condemned. As a rule, the power to swallow is completely in abeyance, and the stertorous breathing is evidence of this fact. Thus croton oil, and even the mouth secretion, find a much easier entrance to the lungs than to the stomach. Let whoever doubts this statement pass a nose-tube on an eclamptic patient, and he will find himself astonished at the ease with which the instrument finds its way into the larynx—an impossible accident under ordinary

circumstances. To ascertain the situation of the nose-tube, all you have to do is to note at what period of respiration air rushes through it. If this occurs during expiration, then the tube is in the trachea; but if during inspiration, the tube is in the stomach, and the air is forced out by the descending diaphragm compressing that organ. It is astounding that this fact has received (so far as I am aware) no notice, for I cannot doubt that the so-called œdema of the lungs, seen with such frequency in fatal cases, is largely to be attributed to fluids which have reached them from the mouth. If this be true, it follows that position must have an important bearing on the proper treatment of these cases. In giving anæsthetics, if fluids form in the mouth, we turn our patients on the side; and so we ought to act in eclampsia, and either prevent our patient at any time lying in the dorsal position, or else employ some suction appliance to keep the mouth emptied of its contents. The gag, too, as usually recommended, and especially in the hands of a nurse, is an instrument fruitful for harm. Physiology teaches us that the act of swallowing cannot be accomplished with the teeth and lips apart. So by inserting this instrument between the teeth any remote chance of the mouth fluids reaching the stomach is entirely removed.

Let me now turn to the surgical treatment for eclampsia—namely, the induction of premature labour, and see to what extent it commends itself as a justifiable proceeding. Its claims to popularity are based on the observed fact that the death of the fœtus and emptying of the uterus, as a rule, materially improve the condition of the patient. It has also recently received commendations from the highest authorities as a prophylactic against the onset of convulsions. As regards its employment as a prophylactic agent, nothing could be more unjustifiable, for the reason that no man can say what case is, and what case is not, going to develop

eclampsia—and when I add that not six per cent. of patients showing premonitory symptoms finally develop the disease, the truth of this statement will be evident. It is an unfortunate fact that the surgical treatment is growing in favour day by day, and this is not surprising, for not only has it the sanction of great names, but it also affords the medical attendant much satisfaction, substituting as it does an active mode of treatment for inactivity with apparent impotence. I cannot but think, however, that it is unsound in theory and disastrous in practice.

I remember Dr. W. Smyly pointing out to me what he believed to be a mistake in the text-books—viz., "that the earlier in pregnancy the fits occurred, the more fatal the disease proved." He rather believes that one of the most serious complications of eclampsia is the onset of labour; and as there is less liability of labour ensuing in the early than the late months, these early cases would usually end in recovery if not interfered with by active treatment. I think a careful study of statistics will fully confirm these views. Certainly, from a theoretical point of view, we should pause before inducing labour, knowing as we do that the danger of the disease is enormously increased by the multiplicity of the fits, and that the fits are easily induced by reflex stimulation. Thus a vaginal examination, abdominal palpation, massage of the kidneys, cold blasts of air, but worst of all, and above all, the dilatation of the cervix, have been observed over and over again to increase the frequency and severity of the attacks. And yet we have to expose our patient to almost all these conditions in order that a very uncertain benefit may follow the emptying of the uterus. Those who support such unscientific methods ought at all events to be able to point to superior results in practice as a justification for them; but these, so far as I know, are not forthcoming. Thus, with your permission, I shall give some figures from Dr.

Green's Report of the Boston Lying-in Hospital, which are excellently compiled and at the same time comparatively recent. They deal with all cases treated in the hospital between the years 1886 and 1892—thirty-six in all—and will be found fully published in the *American Journal of Obstetrics* for June, 1893. The number of cases is, unfortunately, not many, but will be found exceedingly suggestive.

Dr. Green divides his cases into five tables; the fifth deals with fits occurring after labour, and therefore does not concern the present subject. Table I. shows three non-viable cases in which there was no obstetrical interference: all three recovered. Table II. shows six non-viable cases in which labour was induced, with a maternal mortality of 50 per cent. Table III. shows four viable cases in which labour was induced: maternal mortality 75 per cent. Table IV. shows eight interpartum cases with operative interference, upon the usefulness of which there is no disagreement: maternal mortality 25 per cent.

Thus we see that six of the women died on whom labour was induced, and that the mortality actually rose to 75 per cent. when this induction took place with a viable fœtus; while the mortality is *nil* in those cases in which labour did not set in, and was only 25 per cent. in those who were allowed to fall into labour naturally. As far as these figures go, I believe they fully prove the harmfulness of operative proceedings; and what appears to me absolutely astounding is that Dr. Green draws from them arguments in favour of surgical interference.

Dr. Green's statistics have been selected because they are not only recent but complete, as far as they go, and I believe the experience of the Rotunda Hospital will be found to fully agree with my reading of them.

Let me now turn to the method of treating eclampsia that has been pursued in that institution for upwards of

three years, and which I had the honour of carrying out under the present Master. I allude to the hypodermic injection of large doses of morphin. Beginning with the injection of ½ grain, this is followed in two hours by ¼ grain, and so on gradually, until either the symptoms are alleviated or until 2 grains have been given in twenty-four hours. If, in spite of this treatment, labour sets in, forceps are applied to hasten delivery so soon as the os will safely admit their application; but it is held that manual dilatation of the cervix, which in reality means a bursting of that structure, is not a justifiable proceeding. So far as my own personal experience goes, it has been entirely favourable to this treatment, nor is it (notwithstanding the fact that opium is believed to be injurious in most renal diseases) theoretically unsound. The best authorities agree in believing that opium has but little action on either the heart or kidneys, Brunton stating that the secretions from the latter organs at times are increased by its employment. Again, we know of no other drug that will lessen metabolic changes in the tissues to so great an extent, and will thus limit toxin formation. It is a nervous sedative of the first order, diminishing the intensity of reflex impressions, powerfully controlling convulsions and uterine contractions, and, lastly, drying up salivary and bronchial secretions. It is obvious, therefore, that in this drug we possess a means of *counteracting to a great extent* those conditions which tend to kill in eclampsia.

It has been objected that this is a dangerous remedy, viewing it from the standpoint of safety to the child. But this argument can hardly be taken seriously when we remember the very high mortality to the fœtus that obtains, no matter what the treatment may be, and also the bettering of the patient's condition by its death. But, apart from these considerations, I do not believe that morphin can exercise any harmful influence on the child when administered as

above. I have calculated that the child's body at no time can contain a dose of morphin larger than what would be equivalent to four minims of the liq. morph., and when we remember that the inhibitory action of morphin on the respiratory centre of the fœtus does not imperil its existence, it at once becomes evident that a fœtus before birth is much less susceptible to the influence of morphin than a nursing infant would be. All practical experience goes to prove the difficulty of influencing the fœtus before birth by this drug.

Let me now say a few words in praise of blood-letting. Dr. Atthill, in Dublin, has always been a warm advocate for this procedure; and lately Dr. Williams, of London, has urged its employment strongly. It was in general use when the results in the treatment of eclampsia were better than they are now; provided we eliminate the septic cases from the old statistics, and viewing it merely from a theoretical standpoint, there is much to be said in its favour. By abstracting blood we relieve the kidneys, we dry up the bronchial and salivary secretions, diminish venous congestion, and remove an absolute amount of toxin from the body.

If the amount of blood required was abstracted by two operations, with not more than an interval of a couple of hours between them, then the second blood drawn off would likely be richer in toxin, due to the before-named current becoming established in the direction of the blood.

I hold that nourishment in no instance should be given throughout the attack. If hot water stimulants or medicine be called for, let them be administered through a nose-tube, or, better still, per rectum. Purgatives are absolutely necessary, and of other drugs the nitrites or digitalis, in connection with spartein, may, perhaps, be found useful. Hot water may be necessary when the disease has lasted for many hours, and after the second bleeding has been under-

taken, or the convulsions subsided. But if given in the earlier stage of the disease it will do harm by counteracting the necessity of the blood to make good its deficiency in water by an indrawing from the tissues.

In conclusion, I suggest that it is possibly the attention to small details not considered worthy of recording that enables some authorities to show results immeasurably superior to others, though both may apparently pursue a similar line of treatment.

Dr. Horne said, notwithstanding Dr. Tweedy's contribution, he was still of the opinion that as regards the pathology of the disease they were as much in the dark as hitherto. In attributing the disease to toxins, they were, he believed, begging the question, for the presence of those toxins had not been demonstrated. As regards blood-letting, he could not understand how it was a treatment applicable to all cases—for instance, to one patient who was plethoric, and to another who was anæmic. He spoke favourably of the treatment of eclampsia by ½-gr. doses of morphin, or corresponding doses of opium. He also expressed himself in favour of croton oil—a drop being placed on the back of the tongue. He had experience of pilocarpin in one case, and, although in that case he himself did not administer the drug, under its influence the woman rapidly developed œdema of the larynx. He did not assent to Dr. Tweedy's theory as to the elimination of ptomaïns by blood-letting.

The President said that the danger of the convulsions is infinitely greater when they occur in the early stages than when they occur in the later stages of labour. He had induced premature labour successfully in two or three cases. Under certain conditions he would be prepared to adopt the same line of treatment again. However, he regarded such a procedure as a very serious one.

Dr. Alfred Smith pointed out the fact that some German investigators were inclined to believe that acetones in the blood was the cause of eclampsia. The recognised treatment of eclampsia was by large doses of morphin. He followed the practice that obtained at the Rotunda, when he was Assistant Master to that institution—chloroform, purgation, bromide of potassium, &c.—in two cases,

which had occurred in his private practice, and with satisfactory results. They should take their instruction from the large institutions, and when the statistics of such institutions demonstrated the value of opium, the opium line of treatment would be adopted. He would be satisfied with the treatment adopted by Dr. Macan until some better method was discovered.

Dr. Smyly observed that the difference of opinion on the question of the treatment of eclampsia arose chiefly from the habit of forming conclusions on the experience gained of one or two or a dozen cases. No matter what the treatment they had recourse to, sometimes they would get a run of successful cases, and sometimes the reverse. To his mind the question of inducing premature labour or not, was by no means a practical one; for the induction of labour occupies considerable time, and causes great reflex action. He believed chloroform increased the tendency to death. If the patient's death was inevitable, he did not think it was a matter of great consequence whether she died in convulsions or not.

Dr. McWeeney did not think there was anything special about the eclamptic kidney or anything special about the toxæmic condition of the urine. He held with Bouchard that eclampsia was an auto-intoxication. He had not, when examining for Dr. Horne a specimen of eclamptic urine, the means of demonstrating toxins otherwise than by experiments on animals. He said that in eclamptic urine albumen in a greater or lesser degree was always present. A microscopic examination of that fluid invariably revealed hyaline tube casts. Bouchard proved that toxins could be eliminated by acting on the bowels; but the question of the administration of purgatives should be determined by the condition of the patient; and mentioned that small vessels had been found plugged with a tissue, structurally identical with the chorion.

Dr. Lane dwelt on the necessity for prophylactic treatment—dietetic treatment. He thought morphin inferior to pure opium. Dr. Lane made a passing reference to serum treatment.

Dr. Parsons could not consider the treatment by chloroform a rational method, since it was well known that that drug depresses the higher cortical centres. By giving opium they were likewise introducing into the system a substance which exercises a depressing effect on the heart. Opium, however, was a less dangerous drug than chloroform. He failed to understand the advantage to be derived from sweating the patient, in face of the fact that Dr.

Purser had assured him that there was more urea in one drop of urine than in as much sweat as would cover the body from head to foot. He regarded pilocarpin as simply a poison in this disease, since it paralyses the sensorium, already too depressed.

Dr. Macan ridiculed the theory that attributed eclampsia to plugging of small cerebral vessels.

Dr. Tweedy said that success in the treatment of eclampsia largely depended on attention to details. He mentioned many of those details—as, for instance, turning the patient on her side during administration of chloroform.

AN INTERESTING CASE OF RUPTURED TUBAL PREGNANCY, WITH HÆMATOSALPINX OF THE OPPOSITE SIDE.

By ALFRED J. SMITH, M.B., M.A.O., R.U.;
Gynæcologist to St. Vincent's Hospital.

[Read in the Section of Obstetrics, March 6, 1896.]

I MUST ask your indulgence while I read for you notes of an interesting case of ruptured tubal pregnancy:—

Mrs. W., aged thirty-three, married eleven years, had four children, the youngest aged two and a half years; she nursed all her children, and weaned her last child when it was one year and five months old. Her menstruation returned one month after she ceased to nurse, and came on regularly every month; duration, six or seven days, the amount being heavier than before her last pregnancy; she suffered no pain.

The change, which began on December 17th, 1895, was ushered in by a severe pain referred to the bottom of the abdomen, and was accompanied by so heavy a red discharge that the patient was admitted into St. Vincent's Hospital. I saw her a short time after her admission; she stoutly refused to permit any vaginal examination, although her condition was alarming and caused me much anxiety. The rest in bed stopped the hæmorrhage, and the patient felt so well that she left the hospital without permitting me to make a single examination. However, she returned fourteen days subsequently suffering from a return of the hæmorrhage, which came on without any pain. I had no difficulty on this occasion in being permitted to make a vaginal examination. I put the patient under ether and found the uterus

displaced forwards, the fundus not appreciably enlarged. A cystic tumour was easily mapped out in Douglas' pouch, which conveyed the impression of being lobulated. The tumour could not be pushed out of the pelvis; it wobbled from side to side, and it felt quite unlike any other cyst that I had previously examined. I made the diagnosis provisionally of a tubal pregnancy, and urged the patient to undergo abdominal section. Accordingly, on Jan. 29th of this year the patient was placed on Trendelenburg's table and the abdomen was opened. Mr. McArdle kindly assisted me. I found the cyst on the right side bound down by extensive adhesions; on separating these my fingers entered into a cavity full of dark, tarry blood clots which escaped and soiled the pelvis. The amount of fresh hæmorrhage was very slight, as I quickly ligatured the Fallopian tube close to the uterus and the ovarian artery. I then separated the sac and douched out the pelvis with warm saline solution; no trace of a fœtus could be found.

I then examined the cyst on the left side; it appeared similar in character to the one I had just removed but smaller. I was able to separate the adhesions which bound the cyst to the pelvic wall and rectum without its bursting; however, on drawing the cyst up after being separated, it unfortunately burst, discharging a quantity of dark thin blood—no clots. The pedicle was found to be too broad for single suture; I therefore employed an interlocking chain suture, using stout silk. On douching out with saline solution to remove the blood a mass like a small fœtus was seen, but it slipped from my fingers and escaped. An exhaustive search proved fruitless; I could not find it.

The general peritoneal cavity was so thoroughly protected by gauze sponges that the general peritoneum was not soiled. I therefore lowered the Trendelenburg's table and contented myself with just flushing out the pelvis with warm

saline solution, temp. 99° F. I did not sponge the pelvis, or aspirate excess of fluid, but left in a drainage-tube.

The patient suffered very much from thirst, and had so much pain after operation I had to administer 10 grs. of phenacetin; the drainage-tube was removed after twenty-four hours. Recovery was rapid and uneventful.

Remarks.—The chief points of interest seem to me to be—1st. The question of diagnosis. 2nd. The pathological condition found during operation.

The Diagnosis.—There was no history of pregnancy; the menstruation was quite regular up to the very day when she was first seized with the abdominal pain, and heavy menstrual flow. The pain, it is true, was referred to the lower part of the abdomen, but was not so severe as to cause the patient to collapse. She was able to walk to the hospital without assistance. Still, on vaginal examination, nearly all the typical signs of ruptured tubal pregnancy were present.

Pathological condition found at operation.—I am quite satisfied that the condition found on the right side was an example of "tubal abortion." The patient became pregnant in the ampulla of her Fallopian tube, and aborted on Dec. 17th. The large mass of blood clots became encapsuled by protective peritonitis, and was opened into by my fingers while separating adhesions. As is well established, the fœtus is rarely found in tubal pregnancies of this early period.

As to the cyst in the left side, its exact nature is not so clear. It was undoubtedly full of blood, and seemed to be in the outer third of the Fallopian tube, and had certainly all the macroscopic appearances of a tubal pregnancy. But the microscopic examination throws a different light on the subject, and proves it to be not a case of tubal pregnancy. The interesting query now is how to explain the hæmorrhage into the outer third of the Fallopian tube.

Dr. McWeeney has made a most careful examination of

the specimens. His report I have now much pleasure in placing before you :—

"The specimen consists of the two ovaries, with part of each broad ligament and the outer three-quarters of the Fallopian tube of each side. Each tube is enormously dilated towards its abdominal end so as to form a cyst.

"Ovary No. 1 (the right ?) is enlarged to the size of a pigeon's egg. It is dead white, cicatrised, and fibrous externally, whilst internally it is riddled with cysts averaging the size of a pea. These contain proteid material coagulated by the alcohol in which the specimen is preserved. Close beside the ovary is the Fallopian tube, the walls of which are greatly hypertrophied. Towards the abdominal end this opens out into a cyst-like expansion, with walls a quarter inch thick. The convoluted tubal mucous membrane enters this cavity and can be traced for some distance across its inner wall. The whole inner wall is thickly coated with freshly-coagulated blood, which presents a peculiar shaggy villous texture. Externally the cyst is coated—on its peritoneal aspect—with shreds of fibrinous exudation. On the outer aspect of the cyst wall is a large opening, two inches long, bounded by the everted inner wall. Along the margins of this there are fringe-like structures doubtless identical with the tubal fimbriæ. From the appearances just described I have no doubt that this structure represents the sac of a tubal pregnancy cut short by hæmorrhage and subsequent operation at an early stage of development.

"The other ovary is reduced to a mere shell of cicatricial tissue, lying like a cap on the wall of a sort of cyst, which is somewhat smaller than that just described, and the wall of which is much thinner. In this case, also, the cavity of the cyst is continuous with that of the Fallopian tube, the mucous membrane of which opens out on its inner wall. The inside of this cyst is much smoother than that of the other, and shows only a few shreds of inflammatory lymph. Part of the outer surface is coated with similar material. The question as to its nature could be settled only by microscopic examination, and accordingly two pieces were removed, one from a smooth part and the other from a shaggy part of the inner wall, embedded in celloidin and sectioned. The cyst wall was then found to consist of smooth muscle-fibre and connective tissue, covered on the inside with a more or less interrupted layer of high columnar epithelium, part of which could be seen to be ciliated. The shaggy

part of the wall consisted of similar structures, the processes being due to duplicature and infolding of the mucous membrane. The whole of the cyst wall was infiltrated with inflammatory cells, whose fragmented nuclei could be seen even amongst the lining epithelium. Here and there the inflammation had proceeded to necrosis. No trace of chorionic villi or other ovular structures could with certainty be identified.

"*Diagnosis.*—The case from which these specimens were removed presents the highly unusual combination of tubal pregnancy on one side, together with great dilatation of the tube, and hæmatosalpinx on the other. So suggestive of a second tubal pregnancy was the appearance of this latter tube, that nothing but a careful histological examination enabled the diagnosis to be made."

Dr. McWeeney said the dilatation on the left side resembled a case of tubal pregnancy. There were, however, a few points of difference: the walls in this case were thinner than they would be in a case of pregnancy, neither were they coated to the same extent with blood clot. If the contents consisted of clear serous fluid, he (Dr. M'Weeney) would have but little hesitation in describing the case as one of hydrocele. He found pus cells in the walls, from which he inferred that the tumour was inflammatory in its origin.

Dr. Horne thought an interesting point in connection with the case was that the woman had borne four children, and that only two and a half years had elapsed from the time of her last pregnancy. In this case there was no question of sterility—a circumstance so commonly associated with tubal pregnancy. Dr. Smith admitted that with a quantity of blood and serum something like a small fœtus escaped from the cavity; perhaps it actually was a fœtus.

Dr. E. Tweedy thought that it might have been possible that pregnancy had occurred in both tubes, and that on the left side bleeding occurred, the fœtus thus getting surrounded with blood. The tube wall was thin, because there was not sufficient time for it to become hypertrophied. The absence of chorionic villi, he suggested, might be accounted for by the length of time that elapsed from the death of the fœtus. As to treatment, he would ask Dr. Smith whether he would open in every case of hæmatocele, or were there cases when it would be better to put in bed for

three months. In his line of treatment was Dr. Smith guided by circumstances. If so, he (Dr. Tweedy) was anxious to know them.

The PRESIDENT said that the fact that Dr. M'Weeney found pus-cells in the wall of the cyst showed that prior to tubal pregnancy on the right side there was chronic inflammatory disease of the left side. Under any circumstances he would operate in a case of tubal pregnancy. He mentioned the case of a lady who, after the lapse of twenty years, was expelling fœtal bones from her vagina. He (the President) could not assent to Dr. Smith's opinion as to the superiority of phenacetin over morphin.

DR. A. SMITH said he did not attach importance to what Dr. Horne seemed to think might have been the fœtus. As it came away he was under the impression that it was a piece of coagulated albumen. In answer to Dr. Tweedy, he said that whenever he diagnosticated a hæmatocele and formed the opinion that that hæmatocele was due to tubal pregnancy, he operated at once, and did not wait for septic symptoms to arise. There was danger in delay, for septic suppuration might occur at any moment, all the conditions favourable to the development of sepsis being present in those cases. He extolled the virtues of phenacetin, insisting on its superiority over morphin, notwithstanding the President's praise of morphin. He looked upon morphin as a most dangerous drug in abdominal sections. He administered phenacetin in 10 gr. doses.

NOTES OF A CASE OF PULMONARY EMBOLISM; RECOVERY; WITH SOME REMARKS UPON PERIPHERAL VENOUS THROMBOSIS.

By JOHN H. GLENN, M.D.;
Ex-Assistant Physician, Rotunda Hospital, Dublin.

[Read in the Section of Obstetrics, April 10, 1896.]

Mrs. J. was delivered of her second child, a large, healthy boy, on December 9, 1895. The patient was a tall, exceedingly stout, young woman; the labour was natural and not prolonged. When I arrived the head was born and the perinæum required two sutures, which were of silk. She had both before and since her marriage suffered from varicose veins of both legs, and these caused her considerable pain after her first child and also when carrying the second. Her temperature remained normal, and the perineal wound healed kindly, but on the third day my attention was called to the left leg, where I found the internal saphenous vein thickened and inflamed, with a distinct area of redness about the size of a florin. The usual treatment was resorted to—namely, elevation of the limb, suitable posture, lotion of lead and opium, covering with cotton wool, and even bandaging.

The case for three weeks displayed some unusual features, as the temperature in the evening remained below 100° F., and the leg displayed none of the usual hard œdema met with in the so-called "white leg." At this time I felt justified in regarding the case as one of simple venous thrombosis, my only anxiety being presence of obstinate constipation, with white-furred tongue, the well-known recognised prodromata of phlegmasia alba dolens. With this in mind, careful abdominal palpation was practised, but no effusion was to be

detected on either side. During the fourth week the vein was to be felt as a distinct hard cord, but the leg, though painful at night and feeling heavy to the patient, was still quite soft; it was slightly larger than the right on measurement.

Patient and her friends were warned as to the grave dangers incurred by sudden movements, and she was progressing well, if slowly, up to the 17th of January, 1896, or thirty-nine days after her delivery. On the 18th of January she had some visitors, and feeling so much better, probably rather over-exerted herself, for immediately after, when having her leg dressed, she suddenly became faint and breathless, and I was sent for in a hurry. My friend Dr. Flynn, who was kindly doing duty for me, saw her at 9 30 p.m., and found her lying with anxious aspect, rapid breathing, and tumultuously beating heart. Her brother, who is a senior medical student, had, on auscultating before Dr. Flynn's arrival, distinctly heard a systolic murmur, but this disappeared, only, however, to return again with increased volume on her second attack, which took place at 5 a.m. So serious had the case become that Dr. Flynn sent for Dr. Parsons at 6 a.m. Dr. Parsons has very kindly furnished me with the following notes:—"I saw Mrs. J. about six o'clock. She was then rather pale and anxious-looking; there was no distinct cyanosis, or marked distension of the jugular veins; her temperature was slightly elevated; pulse about 110; respiration somewhat increased in frequency, but there was at the time of my visit certainly no dyspnœa. So far as an examination of the front and sides of the chest was concerned, I could detect nothing abnormal in her lungs. I did not care to move her, as I thought doing so might be attended with some danger. As regards her heart there was no thrill or friction palpable, nor was there any marked displacement of the apex beat. A rather rough murmur,

apparently systolic in time, was audible over the mitral area, but was heard with the greatest intensity in the third left intercostal space, about an inch from the left margin of the sternum, and could be traced downwards and inwards towards the end of the sternum. The murmur was loud, single, and had not any of the characteristics of a pericardial friction sound. I recommended digitalis and diffusible stimulants, so as to keep the blood flowing actively, and thereby diminish the tendency of a clot to increase in size."

I myself saw the case at eleven, and found her with a full pulse, beating at 140, temperature 101°, respirations 40. I made a careful examination of her chest and found a well-marked systolic murmur, with its point of greatest intensity over the tricuspid area. The heart beats were regular, but afterwards became intermittent. She was slightly cyanosed when I saw her, but not the livid purple considered as a proof of spontaneous coagulation rather than embolism. The air was entering the lungs, but not easily, as her attention was concentrated upon the act. She was in a most highly nervous state, and had quite made up her mind to die. There was no bronchitis nor pneumonia. I quite agreed with the diagnosis arrived at that it was a case of pulmonary embolism. I have twice before met with it after cœliotomies during my service in the Rotunda Hospital.

The treatment was spts. amm. arom. and spts. ætheris co., with hypodermic injections of morphia to allay her restlessness, which was extreme. Digitalis was exhibited later, and the morphia was changed first to paraldehyde and then to sulphonal, which proved the most serviceable. The murmur persisted for four days, and gradually diminished, while the temperature rose until, on the 22nd, it was 103·4°. Now occurred the most interesting feature of the case. As the murmur passed away a localised area of pneumonic consolidation appeared at the back of the right lung, corresponding to

the middle lobe, while at the same time she began to expectorate rusty-coloured sputum. I submit a chart [exhibited] of this local pneumonia, by which nature expelled this embolus; the amount in the twenty-four hours never exceeded half a spitting-cup. Care was taken to keep her off her back, and the case went on from this time favourably. It is interesting to note that the right leg became affected after this attack, but in merely a slight degree. Dr. Parsons saw her on February 20th, 1896, and writes:—"Her heart seems perfectly normal, but there is still some dulness over the lower portion of the right lung at the back." This dulness has since cleared up, but I believe the pleura is still thickened.

I have brought the above case before the Obstetrical Section, not only on account of the interest which must necessarily attach to a recovery from pulmonary embolism, but also because I regard it as an example of thrombosis apart from sepsis. As long ago as 1877, Spiegelberg stated "that puerperal thrombosis has been far too much mixed up with those complications, and it is desirable that we should study simple uncomplicated thrombosis." An interesting discussion on this point is published in the last volume of the Obstetrical Society of London's Transactions, and the bulk of opinion favoured the non-septicity of peripheral venous thrombosis. In the case here before you I fail to see how it could be included under septic, but should like to hear the opinions of those better qualified to judge.

It must, I think, be allowed to be one of two conditions—either spontaneous coagulation in the right heart, and extending into the pulmonary artery, or an embolus detached from a peripheral thrombus, probably floating out of the femoral vein into the vena cava. I have already mentioned why it is not likely to have been spontaneous coagulation. The great majority of emboli found after death come from the systemic veins, and notably from the internal saphenous

and femoral veins. The reason of coagula forming most commonly in these situations being—first, that the action of the vis a tergo and thoracic aspiration is scarcely at all felt; and, second, the lying-in state is exceedingly favourable to this condition on account of the state of the blood, the dilated pelvic and femoral veins, and the diminution of the heart's action; while, if we consider these same causes as provocative of varices before delivery, we must add the general rise of abdominal pressure consequent upon the increased dimensions of the uterus. In this puerperal complication it is satisfactory to note that many more recoveries are reported than perhaps is generally recognised, and it is a great satisfaction to me to be able to add yet another.

Dr. Parsons said the characters of the friction sound were endocardial and not pericardial. Thrombosis might have occurred in the right auricular appendix, and have produced the murmur. He thought that more probably it was due to an embolus. It was very remarkable that in a month afterwards the heart sounds were perfectly normal. He saw a *post-mortem* on a woman in Vienna who had got up three days after confinement. A large embolus was found completely obstructing the pulmonary vessels.

Dr. E. Tweedy said pulmonary embolus was a very rare affection. He did not think it would be commoner if women got up six hours after confinement. The orthodox treatment was to give plenty of stimulants, as the patients generally died of shock. In this case, as the woman was not suffering in any way, large doses of ammonia and digitalis might have done harm by washing away the clot into some more dangerous situation.

Dr. Doyle said that some years ago he had brought a patient to the Obstetrical Society who had several attacks of severe dyspnœa after confinement. She was treated with carbonate of ammonia and ether, from which she derived the greatest benefit, and finally made a good recovery.

Dr. Kidd had met with only one case of pulmonary embolism. It occurred a few hours after delivery. The patient died in spite of treatment by stimulants. Some time ago five cases of pneumonia

occurred in the Coombe Hospital; three of them died, but no trace of septic trouble or of embolism could be found as the cause of the pneumonia. He thought one of the cases was saved by oxygen inhalation.

Dr. Winifred Dickson thought that inhalations of oxygen would be of more use in combating shock and dyspnœa than stimulants. It could be given more quickly. It was very good for relieving dyspnœa in pneumonia and heart cases, and it might be of use in embolus.

Dr. Smith said the chief point of interest was whether this phlebitis was septic or not. He classified thrombosis under two forms. First, passive thrombosis; it frequently occurred in persons of feeble circulation; the temperature was rather subnormal. Second, where a local inflammatory trouble was found, there was a rise of temperature, and it was generally associated with some form of septic organism. Dr. Glenn's case was not one of passive thrombosis. The septic organisms, however, were not streptococci or staphylococci, for then the woman would never have got well.

Dr. Glenn, replying, said that only 10 minim doses of digitalis were given twice a day. He gave diffusible stimulants, especially ammonia, because ammonia was supposed to have some efficacy in preventing coagulation of blood, and in helping the resolution of blood clots already formed. Oxygen would act quickly, but would not assist the resolution of blood clots. The temperature in this case never rose above 100° F.

HÆMATOMA OF THE VAGINA OCCURRING DURING LABOUR.

By H. WILSON, L.R.C.P.;
Senior Assistant Master, Rotunda Hospital.

[Read in the Section of Obstetrics, May 15, 1896.]

THE case which I have the pleasure of bringing before the Academy occurred in the practice of the Rotunda Hospital last year.

E. B., aged 23, 1-para, came into labour on the evening of the 23rd Feb., 1895. She was attended by the pupils of the hospital during the night, who noticed nothing unusual till 10 a.m. the following morning, when the left labium majus was observed to be swollen. As the swelling was increasing, the Extern-Assistant, Dr. Murphy, was summoned, and he, recognising the case as one of hæmatoma, sent for me, as the Assistant on duty. On examination, I saw a large swelling, bluish in colour, occupying the L. ischio-rectal region, extenting up the L. labium, distending the perineum, and bulging into the vagina. The contour of the tumour was very uniform, and presented the appearance of a fœtal head, greatly distending the part. The vulva was pushed over to the right side. The child's head was presenting, and about midway in the pelvis. As the patient was getting good pains, and the swelling was increasing, it was thought better to deliver her at once. Forceps were applied, and the delivery effected easily; unfortunately the tumour burst the moment traction was made, and a large quantity of blood came away with the child. There was no difficulty with the placenta, and the uterus contracted well, but as the bleeding continued, the patient was placed in the dorsal position (cross bed) for examination. The perineum was considerably lacerated, and two veins were seen to be bleeding profusely. These were at once ligatured. The rent in the vagina extended from the perineum up the left wall of the vagina, almost to the lateral fornix. No definite points of bleeding could be made out in this situation, but still there was considerable venous oozing. The vagina and cavity

left by the effused blood were thoroughly douched with hot creolin solution, which checked the bleeding, and firmly packed with iodoform gauze and sterilised cotton wool. A large pad was placed over the vulva and perineum, and a tight binder and T-bandage applied. Owing to the patient's surroundings, it was thought better to bring her into hospital. She was a good deal collapsed, but, with treatment, soon came round. The tampon was removed in 30 hours. There was no hæmorrhage. The patient made an uninterrupted recovery; her temperature only once rose to 100° F. Owing to her anæmic condition, however, she was kept in hospital three weeks. I may mention that she was delivered last month of her second child, which was born without any difficulty.

In connection with this case, a few remarks in reference to hæmatomata of the vagina may not be out of place. They may occur in the non-pregnant as well as in the pregnant woman, and in the latter may form before, during, or after labour, tumours forming during labour being by far the rarest, of which the case now reported is an example. The immediate cause is the rupture of one or more of the blood-vessels in the walls of the vagina or the labia. The predisposing causes in labour cases, to which I confine myself, are very obscure. Unfavourable position of the child's head, protracted labour, the use of forceps, pendulous abdomen, &c.—in fact, any condition which causes undue pressure or traction on the cervix or vaginal walls undoubtedly favours their development. A varicose condition of the vaginal veins would also predispose, but this condition is by no means constantly present. Winckel states that it occurs in only 12 per cent. of these cases. As regards the time of the occurrence of the rupture, blood tumours developing before labour are, as a rule, traumatic in origin, and occur shortly after an injury; while in those recognised both during or only subsequent to delivery, the rupture of the vessel probably occurs at the same stage of labour—it may be when the head is still in the brim. If the point of rupture is pressed upon by the child's head, which it usually

is, bleeding can only take place when this pressure is removed by the further descent of the head, or by the birth of the child. In some cases, however, the rupture only takes place during the first few days of the puerperium, and is then probably due to necrosis of a vein from prolonged pressure.

This complication is rare. Winckel estimates its frequency at 1–1,000, Hugenberger at 11–14,000, and Sinclair and Johnston 7–13,000. In seven cases which I have been able to get details of five were diagnosed after delivery, and one ended fatally. The extent of these tumours varies greatly, as a rule from the size of a hazel-nut to that of a tennis-ball; but they may become enormous, filling the small pelvis and extending far up into the abdomen.

The two great dangers in these cases are hæmorrhage and sepsis. Hæmorrhage, where the tumour either ruptures spontaneously or is opened before the bleeding vessel has become occluded by clot, or, in the case of the large tumours, death may occur even before the patient has lost a drop of blood externally. Sepsis has to be guarded against by the most scrupulous cleanliness, more especially in those cases where it is found necessary to pack the cavity.

The treatment of this accident depends upon—firstly, the stage when it is diagnosed; and, secondly, the size of the tumour.

Should a hæmatoma be recognised before the birth of the child, delivery should be accomplished as quickly as possible. If the tumour causes obstruction it should be opened by a free incision in the most dependent part, emptied of blood and clot, and, after extraction of the child, any bleeding points seen should be secured, and the cavity and vagina packed as tightly as possible, in order to prevent any further hæmorrhage. Once the tumour is opened, however, delivery must not be delayed, as, until the child is born, it is

not possible to control the bleeding. which may be fatal in a very short time, as in a case reported in the *B. M. J.* for 1879.

In those cases only recognised after delivery, when the tumour is small, pressure of a colpeurynter, or tight packing of the vagina, may suffice to prevent its extension; should, however, the tumour be increasing in size, it should be opened freely, the bleeding controlled, and the cavity packed, as before mentioned. The only other indication for opening is suppuration occurring in the tumour, the after-treatment consisting in daily tamponing the cavity, and douching as long as any pus appears on the plug. In these cases care should be taken that the discharge does not gain access to the uterus.

CASE OF SUPPURATION IN AN OVARY, THE RESULT OF DIRECT VIOLENCE.

By R. D. PUREFOY, F.R.C.S.;
Obstetric Surgeon, Adelaide Hospital.

[Read in the Section of Obstetrics, March 6, 1896.]

WHEN we have regard to the anatomical structure and marvellous functional activity of the ovaries in a healthy woman, and how easily the regular performance of these functions is disturbed even by trifling emotional causes, no surprise will be felt at the frequent occurrence of disease in these organs. It will, I think, be admitted that even acute inflammation of an ovary seldom terminates in suppuration, except in cases of puerperal sepsis, and also in cases where gonorrhœal vaginitis has been the origin of the mischief. The view has been advanced that in some cases of protracted and difficult labour the ovary may be subjected to injurious pressure, and most of us have seen patients who could trace back to some particular lying-in the beginning of the pain and suffering which so often attend chronic ovaritis. The brief history which I proceed to lay before you will serve to demonstrate that mechanical pressure, if considerable in degree and suddenly applied, may bring about inflammatory mischief in an ovary, and such as may end in suppuration and the structural disintegration of the organ.

In February, 1895, a young woman, aged twenty-one, in good condition and of healthy aspect, sought my advice for various pelvic and menstrual troubles, and gave me the following history:—She had been earning her bread as a governess, and in October, 1891, one of the children in her charge jumped on her as she lay in bed, hurting her consider-

ably with her feet, and causing her much pain in the left ovarian region and in the left leg. Immediately afterwards she became unwell for some days, and during the succeeding four years, till she came under my care, menstruation continued profuse, irregular, and attended with so much pain in the lower belly and left leg, that much impairment of her general health ensued. For the relief of these troubles she was sent to me by my friend, Dr. Wynne.

A vaginal examination enabled me to detect an enlarged and much-retroverted uterus, and the left ovary enlarged, very sensitive to pressure, and somewhat prolapsed. The right ovary appeared to be normal and healthy. I explained to her that nothing save the removal of the injured organ would ensure any permanent relief to her sufferings, and she readily consented, and shortly afterwards was admitted to the Adelaide Hospital for the purpose. The operation was followed by an easy convalescence, and menstruation now recurs regularly without pain, and normal in amount, though for some months the patient complained occasionally of rather severe pain in the left side and left leg.

The PRESIDENT regretted very much that they had not before them the report of a pathologist on Dr. Purefoy's case, as suppuration of the ovary was a very rare occurrence indeed.

SECTION OF PATHOLOGY.

TRAUMATIC RUPTURE OF THE HEART NOT IMMEDIATELY FATAL.

By E. H. BENNETT, M.D., F.R.C.S.;
Surgeon to Sir P. Dun's Hospital;
Professor of Surgery, Trinity College, Dublin.

[Read in the Section of Pathology, November 1, 1895.]

THE interest that attaches to injuries of the heart that are not immediately fatal justifies my submitting this specimen to the Royal Academy of Medicine. The history of the case is this:—Last summer, on July 2nd, about 10 15 a.m., a young man was carried into Sir Patrick Dun's Hospital. He was brought from Cumberland-street, a distance of nearly one-third of a mile, from a house in which there had just before occurred an explosion by which he was knocked down, and the glass of the window of the room in which it took place was in great part blown out. I happened to be in the hospital theatre when he was carried in. He was laid on the table in a condition of profound collapse, but conscious and capable of understanding what was said to him. He was very sullen and would not answer any question. His left wrist was burnt, and his left breast was burnt and blackened with gunpowder; at the level of the left 5th rib close to the sternum was a lacerated wound, which was, however, only skin deep. There was no penetrating wound of the chest-wall. As we stripped his clothes a great quantity of No. 4 shot fell out on the floor from amongst the folds of his clothing, which were burnt and torn around the seat of

injury. His temperature was not taken. His pulse was 120, and he was evidently in great pain. The examination of the injured area of his chest, extending from the 2nd to the 7th rib, and from the anterior axillary line to the left margin of the sternum, gave osseous crepitus to such an extent that the sounds quite masked the heart sounds. There was no emphysema of the areolar tissues, and the percussion sound was but slightly, if at all, altered from normal; the respiratory murmur was also heard at moments over the wounded area—facts that excluded the diagnosis of pneumo or hæmothorax. Nothing positive could be determined except the existence of many fractures of the ribs and their cartilages in the wounded area. The man lived just two hours after his admission, and just before his death became so violently delirious that he had to be held down in bed.

It was found that a tin canister had exploded in his hands and had caused his injuries. He was employed in night work as an employee of the Cleansing Department of the Corporation, and had just before the explosion been before the fire waiting for his breakfast, No one but himself was in the room when the explosion occurred, nor could anyone tell whether he found and brought home the bomb, or whether he had manufactured it himself. He had sometime before been for a short time in a lunatic asylum. It is worth noting that July 2nd was the day on which the Lord Lieutenant was to make his entry into Dublin. The following is the note of the *post-mortem* examination:—

On opening the thorax it was found that the 3rd, 4th, 5th, 6th, and 7th ribs were broken along the anterior axillary line. The cartilages of the 4th and 5th ribs had been partially dislocated from the sternum, and the 5th cartilage was fractured at its junction with the rib; the 5th rib presents in all, bone and cartilage, four fractures. The right lung was healthy, but adherent to the chest-wall. The left was

also completely adherent and was bruised and infiltrated with blood. The pericardium was distended, and when punctured clear serum escaped in quantity and afterwards blood coagula. There was no wound of the pericardium. The heart was wounded in the anterior coronary groove, and a branch of the anterior coronary artery was torn. At the apex of the heart and beside it on the anterior border the left ventricle was opened by two parallel rents, which are very oblique. It is clear that the obliquity and small size of the openings in the left ventricle and in the artery allowed of the escape of blood into the pericardial sac to take place very slowly.

The case which I have found most like this among the records of injuries of the chest is the following:—

Macleod (in his notes on the surgery of the war in the Crimea) mentions the case of a man "hit by a round shot on the edge of the breast-plate, which was so turned inward as to fracture the cartilages of the fifth, sixth, and seventh ribs on the left side close to the sternum. The skin was not wounded. He walked to the rear and complained but little for two hours, when he was seized with acute pain in the region of the heart. His pulse became much accelerated, and he grew faint, and collapsed. A distinct and sharp bellows-sound accompanied the heart's action. He died in seventy-two hours from the receipt of the injury, the pain and dyspnœa which had been so urgent at first having abated for some hours before death. The heart was found to have been ruptured to an extent sufficient to allow of the finger being thrust into the left ventricle. The obliquity of the opening had prevented the blood escaping into the pericardium, which contained about two ounces of dark-coloured serum."

Mr. F. A. Nixon had had a case of wound of the heart resembling Dr. Bennett's in some points. The man stabbed himself over thirty times in the region of the heart, quite a number of the wounds

penetrating the heart substance, but only two entering its cavity. He also was difficult to keep in bed. He lived nearly twenty-four hours. The patient used to suffer from pain about the umbilicus. At the *post-mortem* he found the pancreatic duct filled with calculi. He wished to know the cause of the white serous fluid in Dr. Bennett's case.

Dr. Fraser thought that the distension of the pericardium brought about the delirious state, just as occurred in cases of rheumatism.

The President said that in any cases of aneurysm rupturing into the pericardium there was no clear fluid. In a case he saw recently the pericardium was full of nearly fluid blood, although the person had died several hours previously.

Dr. Bennett, replying, said that the man was not drunk. There was no penetration of the pericardium from without. He never before had seen the pericardium distended with blood which was differentiated into serum and clot, as it was in this case. It was not perfectly transparent serum, but rather opalescent. The wound of the coronary vessel communicated with the cavity of the pericardium, but it was very small.

ON KIDNEY TUMOURS DERIVED FROM SUPRARENAL "RESTS."

By EDMOND J. McWEENEY, M.A., M.D.;
Professor of Pathology and Bacteriology, Catholic University Medical School;
Pathologist to the Mater Misericordiæ Hospital, Dublin.

[Read in the Section of Pathology, November 29, 1895.]

My attention was first called to the subject by the examination of a tumour of this kind, which my colleague, Mr. Coppinger, successfully removed by nephrectomy in March, 1892. Another such case having been operated on at the Mater Misericordiæ by my colleague, Mr. Lentaigne, in September, 1895, I resolved to publish both in detail, and at the same time to collect from various sources our knowledge of this class of tumour, which, so far as I can ascertain, does not seem to have received from British pathologists the attention which its well-marked characters undoubtedly deserve.

Case I.—Mrs. B., a thin, delicate-looking woman, aged fifty-three, mother of seven children, had suffered for nine months from night sweats, vomiting, and abdominal pain. She had been operated on by Mr. Reeves at the Soho Hospital for movable kidney. Further operation for the removal of the organ was proposed and declined. There was now a tumour on the left side of the abdomen; no hæmaturia. On March 4th, 1892, abdominal section was performed by Mr. Coppinger, and a smooth tumour as big as a large melon exposed in the situation of the left kidney. It fluctuated, and after an unsuccessful attempt to aspirate its contents, it was slit open and a bowlful of brain-like hæmorrhagic material evacuated, Mr. Coppinger remarking that all previously reported cases of soft sarcoma of the kidney which had been opened had proved fatal by hæmorrhage. The cavity at once filled with blood, but the

hæmorrhage was successfully dealt with and the tumour removed. The patient, who recovered without a bad symptom, is still alive and well.

Naked-eye Appearances.—The specimen consists of three-fourths of a left kidney, the lower fourth being merged into the walls of a sort of cyst, the inner side of which is covered with pultaceous and hæmorrhagic shreds of tumour substance. There is a distinct fibrous partition between kidney and cyst.

Microscopic Characters will be dealt with together with those of the second case.

Case II.—Patient, female, aged thirty-eight, admitted in September, 1895, to the Mater, under the care of Mr. Lentaigne. Had been suffering for six months from abdominal pain and a tumour in left side of abdomen. No disorders of micturition. Patient looked cachectic and emaciated. Laparotomy revealed a large lobulated tumour of the left kidney extending to the middle line. The size and connections of the growth rendered the operation very difficult and prolonged. Death followed in nine hours from shock. Necropsy refused.

Naked-eye Appearances.—The mass weighed 1,100 grammes, and consisted of a mass of confluent encapsulated nodules, from the upper part of which projected the tip of the left kidney. The colour and consistence of the nodules were not unlike those of kidney cortex. The dimensions were 21 cm. × 16 × 8·6. The diameter of the largest nodule was 9 cm. The centre of each of the larger nodules was occupied by a necrosed cheesy-looking mass as big as a filbert, and here and there were yellowish degenerate patches as big as a pea. Elsewhere the tissue looked free from retrogressive change, and there were no cysts. The encapsulation of each nodule was perfect and definite.

Microscopic Characters.—To avoid repetition, the two cases will be described together. The structure of both was the same, and consisted of (*a*) rows and (*b*) groups of large (7 to 12 μ wide by 12 to 20 μ high) columnar or cubical cells of

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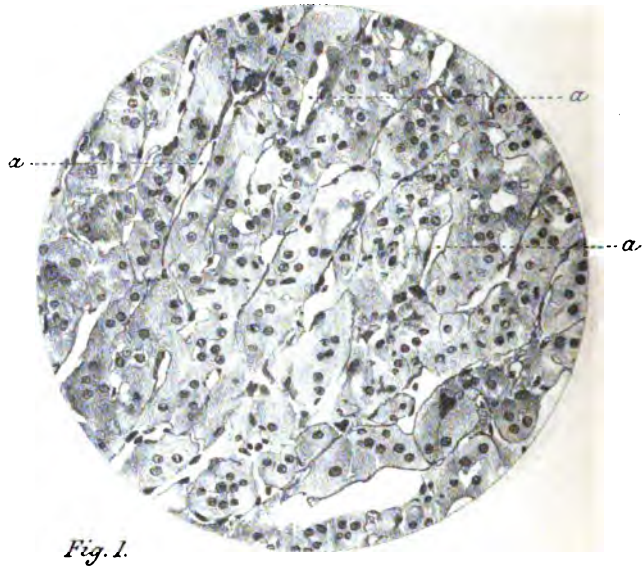
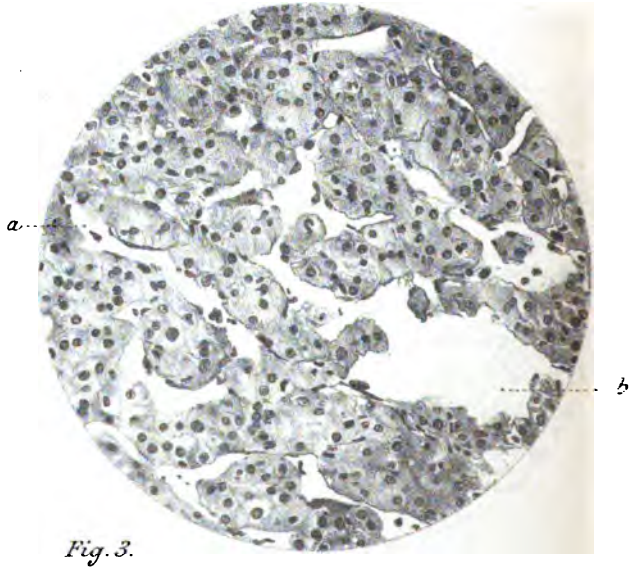


Fig. 1.



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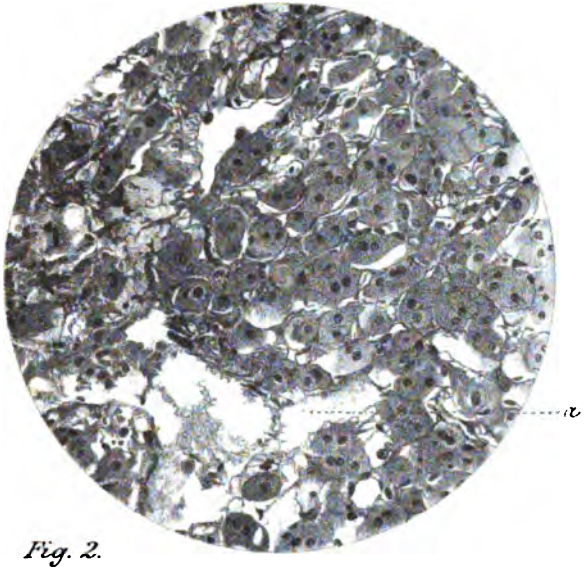

Fig. 2.

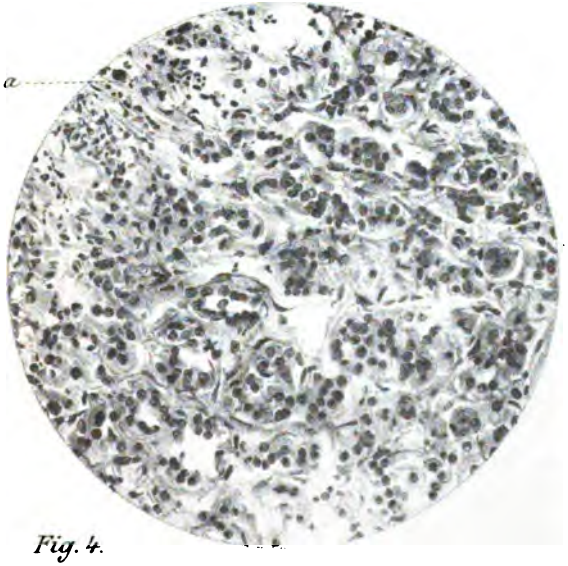

Fig. 4.

F. Huth, Lith^r Edin^r

obviously epithelial character with clear, sometimes reticulated, protoplasm and a relatively small, round, not very deeply staining nucleus, in which a distinct nucleolus was generally visible. Their general appearance was like that of liver cells. The rows in which they were arranged were usually two cells deep, and there was usually no trace of a lumen. On each side of the rows was a line of delicate endothelial nuclei evidently forming the wall of capillaries in which remains of blood could still here and there be traced. Elsewhere the capillaries were collapsed, and then the adjacent cell rows came into contact with each other. Between the epithelial cells comprising these rows there entered no stroma whatever. In fact, no stroma existed, the tumour cells being placed directly on the endothelium of the capillaries. Distinct fibrous trabeculæ divided the tumour substance into lobules, and supported the larger vessels. All transitions could be traced from lengthy rows or double columns to circular groups of cells (see Figs. 2 and 3).

Towards the degenerate parts, many of these groups contained a central space or pseudo-lumen, as I propose to term it, and might readily be mistaken for gland tubules. The cells, however, in these situations are flattened or cubical rather than high-columnar; the nucleus lies close to or may actually project into the "lumen;" the latter is irregular in outline, running out, so to speak, into gaps formed by the partial disappearance of one or more of the surrounding cells; and lastly, serial sections show that the "lumen" ends blindly in the continuity of a cell column, or seems to twist out between the cells. For the four reasons just given, I consider that these central spaces are due to degenerative or metaplastic change, and are not to be regarded as the lumina of gland tubules. As we advance further into the degenerate regions, we find the cell groups becoming smaller, the protoplasm more swollen up and reticular, the nucleus poorer in

chromatin, and showing signs of impending fragmentation (karyorrhexis of Klebs), until in some fields we see only a few enormous cells embedded singly, or in twos or threes, at wide intervals in structureless, hyaline, or hæmorrhagic ground substance.

The tissue that filled the cyst in Case I. differed from the type just described in the fact that the extensive hæmorrhagic and fatty changes of which it had been the subject somewhat obscured the structure. Extensive deposition of glycogen, in the form of intracellular granules and largish extracellular masses, had occurred in Case I. Case II. showed no trace of glycogen; fat globules, however, occurred in many cells, and in the paraffin preparations were represented by large vacuoles.

The neighbouring kidney tissued showed atrophied or dilated tubules lined with flattened, confluent, badly-stained epithelium, shrunken or hyaline glomeruli, blood vessels with thickened walls, and, between these, tracts of connective tissue crammed with nuclei. The septum which everywhere intervened between kidney and tumour tissue (see Fig. 1) consisted, on the tumour side, of dense laminated connective tissue rather rich in nuclei, and, on the kidney side, of atrophied and compressed renal structures. Nowhere could a transition from urinary tubules to tumour tissue be traced.

Diagnosis.—The resemblance of the sections to the tissue of the suprarenal cortex was evident and unmistakable. Zona fasciculata and glomerulosa were both represented—see arrangements (*a*) and (*b*) above. I consider that these tumours are really of suprarenal origin, for the following reasons:—

1. The appearance of the cells, their arrangement in rows and groups, and their relation to the capillary wall, agrees with that of the suprarenal, and differs altogether from the kidney.

2. The presence of large fat globules in many of the cells,

coupled with the absence of indication of degenerative change. This fatty infiltration is a normal condition in the adult suprarenal, and here, as in the tumour, is associated with abundant vascular supply.

3. The fact that aberrant nodules of adrenal tissue have been described by Klebs,[1] Schmorl,[2] Newton Pitt,[3] and Rolleston,[4] as occurring imbedded in the kidney, or flattened against its capsule.

4. The fact that tumours of precisely similar structure not infrequently originate from the suprarenal capsule, and growing downwards into the kidney cause its destruction (Morris[5]).

Granting that these tumours are really so derived, we have next to consider the question of nomenclature. Are they sarcomata, carcinomata, adenomata, teratomata, or what? The term angiosarcoma actually applied by de Paoli[6] to a tumour of this class might to some seem applicable. Driessen[7] deliberately rejects (on manifestly inadequate grounds) the suprarenal origin of two growths, which from his illustration and description clearly belong to this category, and calls them endotheliomata. He regards them as originating from the endothelium of lymph-clefts. Sudeck,[8] by an elaborate chain of argumentation, in the course of which he claims to have traced the transition from urinary tubules to tumour tissue as above described, seeks to class them with renal adenoma. The consideration of these views must be reserved for another place.

The following characters seem to point to carcinoma:—1. The cells are obviously "epithelial." 2. The stroma does not penetrate between them, but divides the cell groups into lengthy or circular alveoli.

I take the "stroma" of these tumours to consist merely of capillary blood vessels—a stroma "reduced to its lowest terms." Now since the adoption of Waldeyer's[9] and

Thiersch's [10] views we must, in diagnosing carcinoma, take into account the development of the tissue from which the tumour originates. If mesoblastic, sarcoma; if epiblastic or hypoblastic, then adenoma or carcinoma. Balfour, and Mitsukuri,[11] and Braun,[12] derive the adrenal from accumulations of connective tissue cells which are formed at the anterior part of the primitive kidney (Wolffian body) along the inferior cava and cardinal veins. Janosik,[13] Weldon,[14] and Mihalkovics [15] regard these formative cells as derived from the epithelium of the body cavity.

Concerning the exact point of origin, however, two distinct views are held. Janosik and Mihalkovics divide the suprarenal from the germinal epithelium in the anterior part of the genital ridge. Weldon, on the other hand—and his view is the one favoured by Hertwig [16]—brings the suprarenal body into connection with the most anterior part of the primitive kidney. When the "sexual cords" at the head end of the primitive kidney sprout out of the epithelium of the Malpighian glomerules, they divide into two branches, one of which turns ventrally into the fundament of the sexual gland, whilst the other turns dorsally, and spreads out in the vicinity of the vena cava. Hertwig goes on to point out how Marchand's [17] observations of accessory suprarenal bodies at the margin of the broad ligaments are explained by this genesis; small portions of what is to be suprarenal cortex appear sometimes to detach themselves, and remain in the vicinity of the sexual organs, in the migrations of which they participate. Again, the researches of Semon [18] place the adrenal in close developmental relation with the mesonephros or primitive kidney, which would to some extent account for the distribution of adrenal "rests" through the substance of its successor, the metanephros or permanent kidney. The cœlomic "epithelium" being a mesoblastic structure, it follows that the suprarenal cortex is also mesoblastic. Hence,

according to the received method of tumour nomenclature, malignant neoplasms originating therefrom would be classed as sarcomata. But Waldeyer himself, the originator with Thiersch of this system, defines[19] epithelium—for tumour classification purposes—as comprising all those cells "which arise from the epiblast or hypoblast, or from the so-called germ epithelium."

Now, the germ epithelium is part of the cœlomic epithelium, from which competent embryologists (*vide supra*) derive the connective tissue cells (Hertwig) or formative cells which (*vide supra*) give rise to the suprarenal body. Therefore, the cells comprising the suprarenal come to be epithelial in Waldeyer's meaning, and tumours arising from them are adenomata or carcinomata. Again, Graf Spee[20] claims to have traced epiblast into the primitive kidney, and if this view be accepted in connection with Semon's results, the growths in question might be classed as carcinoma *sensu stricto*. As a matter of fact, many parts of Case II. diverge more or less markedly from the suprarenal type, and tend to take on a carcinomatous structure (see Fig. 5). A glandular appearance and arrangement of the cells in the normally-situated suprarenal cortex has been described by Creighton,[21] Pfaundler,[22] Marchand,[23] and others, and may be brought into an interesting parallelism with Pflüger's tubules in the ovary, which are also derivatives from the cœlomic epithelium. To sum up, we have to deal with cells mesoblastic in origin and nevertheless epithelial both in appearance and arrangement—a striking example of the way in which the two criteria accepted now-a-days for tumour classification may clash with one another. Developmentally these growths are mesoblastic; morphologically they are epithelial. Precisely the same difficulty applies to other kidney tumours. Those who speak of "carcinoma" and "adenoma" of the kidney tacitly drop the developmental

and adhere solely to the morphological criterion—a proceeding which would, if logically pursued, place the alveolar sarcomata and the endotheliomata amongst the carcinomata, as Hansemann actually proposes.[24] In view of their widely different clinical aspect, such a course might, as Lubarsch [25] points out, be inadvisable. Until, therefore, tumour classification finds its way out of the *impasse* in which the attempts to combine histogenetic with morphological criteria have landed it, we must hesitate to rank the neoplasms now under discussion with either the sarcomata or the carcinomata. For the present I prefer to call them "kidney tumours derived from suprarenal 'rests,'" discarding Grawitz's name, *strumæ lipomatodes aberratæ renis*, as equally cumbrous, more obscure (*strumæ*), and not always (*lipomatodes*) applicable.

HISTORY OF THE SUBJECT.

Grawitz[26] was the first to derive renal tumours from suprarenal "rests." He worked out his views in the first instance on some specimens preserved in the Berlin collection under the name of renal lipomata, and describes in detail two cases of large tumours, one of which had formed metastases in the lung, and the structure of which is identical with those now under consideration. He proposes to call them "strumæ lipomatodes aberratæ renis," basing his nomenclature on the fact that Virchow, comparing localised enlargements of the suprarenal with those of the thyroid, calls the former adrenal "goîtres" (strumæ). Strübing,[27] Löwenhardt,[28] Beneke,[29] Ambrosius,[30] and Horn,[31] take the same view, and add fresh cases. The latter generalises on Grawitz's position, and classes Klebe's[32] and Sturm's[33] adenomata of the kidney, Weichselbaum and Greenish's[34] "papillary and alveolar adenomata," and Sabourin's[35] *épithéliomes métatypiques*, as well as de Paoli's[36] primary angiosarcoma of the kidney in the same category. Askanazy[37] adds a first-rate case, and

accepts Grawitz's explanation. Meanwhile Driessen[38] and Suddeck[39] describe cases which clearly are of this kind, but, as above stated, reject the suprarenal origin. Lastly, the whole question receives a masterly discussion at the hands of Lubarsch,[40] who concludes in favour of Grawitz's opinion.

GENERAL CLINICAL CHARACTERS.

I will now briefly summarise the main points of arguments offered by the thirty cases collected last year by Lubarsch, and point out in what respects our Mater cases agree or differ from the other.

Sex.—Out of 29 cases cited by Lubarsch 20 were those of males. Both our Mater patients were females.

Age.—Out of 29 cases 21 were between 40 and 79; 8 between 2½ and 39. One of our cases was 53, the other 38.

Side.—The right kidney was affected 12 times, the left 11. In both of our cases the left was the seat of the disease.

Duration.—This is usually long, two, six, and even ten years elapsing from the first clinical symptoms. Askanazy's patient felt the tumour when very young. It disappeared for a time, and returned fatally when he was 53. They grow slowly at first, and then seem suddenly to produce cachexia and prove fatal.

State of Urine.—This seldom yields positive evidence of the existence of the tumour. It did so, however, in Grawitz's first case, though the indications were susceptible of misinterpretation.

Result of Operation.—This is unusually fatal. Madelung's 3 cases reported by Lubarsch died without uræmia or peritonitis to account for the fatal termination. Askanazy's case died from recurrence. One of our Mater cases (Mr. Coppinger's) is still alive, three years and a half after operation, and is one of the very few recorded cases in which the patient was cured by the operation.

Metastases.—These were found 20 times out of 29—a fact which proves the malignant tendency of these suprarenal neoplasms. One of our cases having recovered, and the necropsy of the other having been refused, I am not in a position to say anything on this head. Generalisation is usually ushered in by rupture of a tumour mass into the renal vein. The lung is a favourite seat of the secondary growths. They have also occurred in the liver, pancreas, heart, thyroid, &c.

British Cases and Literature.—In the debate [41] on the case of a kidney tumour which seems to belong to this class, and which was operated on by Mr. Walsham, Dr. Newton Pitt expressed the opinion that some renal tumours were possibly related to aberrant suprarenal bodies. The patient in this instance was a child, aged nine months. If my suspicion as to the true nature of this malignant growth, which recurred and caused the patient's death eleven months after Mr. Walsham's successful operation, prove correct, this will be, perhaps, the earliest known instance in point of age. On the same occasion Dr. Snow spoke of a collection of 60 tumours derived from the Wolffian bodies having been made by Mr. Windle. I do not know where the account of this collection is to be found. Mr. Shattock [42] suggests the Wolffian origin of many kidney growths. Hamilton [43] calls tumours of this kind "hæmorrhagic sarcoma." Cases recorded by Morris,[44] Sharkey,[45] Paul,[46] and others may possibly prove to belong to this category, as most certainly does Bell and Johnston's case.[47] In Mr. Bland Sutton's work on tumours there is no mention made of the relation of *epithelial* kidney tumours to suprarenal "rests." I heartily agree with the author when he says (p. 257) that "it is impossible to write an account of carcinoma of the kidney until surgeons are more alive to the interest of the question, and take steps to place malignant tumours of the kidney whilst still fresh in the hands of

experienced pathological histologists." Enough has been said to show that the English literature of surgical pathology is almost a blank on this important and interesting subject; and the object of this paper is to attract the attention of British pathologists to these little-known tumours, and to do something towards placing our knowledge of them on the same level as that of most other neoplasms. My best thanks are due to my colleagues at the Mater, Messrs. Coppinger and Lentaigne, for kind permission to utilise their specimens and clinical notes.

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EXPLANATION OF PLATE.

FIG. 1—"Fasciculate" arrangement of tumour-cells resembling that found in suprarenal cortex; (*a*) Capillaries.

FIG. 2—Arrangement resembling the *zona glomerulosa* of the suprarenal cortex.

FIG. 3—"Fasciculate" arrangement with irregular dilatation (*a*) of capillaries and partial break-down (*b*) of tumour substance.

FIG. 4—Carcinomatous arrangement of tumour-cells; (*a*) "Mast"-cell in a necrosed area.

[From micro-photographs by Mr. A. Pringle].

DR. O'SULLIVAN said he believed the paper was the most complete description in English of these tumours. When in Vienna last summer he had seen two of these growths. The first was like Dr. McWeeney's first. It was an encapsuled mass, confined to the upper part of the kidney, and consisted of pultaceous material infiltrated with blood. There was no metastasis. No microscopic examination was made. The other was of more pathological importance, since in the kidney itself there were several masses of the same character, but also a considerable amount of metastasis in the bones. A piece of the tumour in one of the bones was taken and pressed out on a slide. The cells were arranged in fasciculi. The hæmorrhages seemed to be very frequent, and might be compared to the hæmatomata so frequently met in the suprarenal. He could not imagine these tumours spoken of as lipomata.

THE PRESIDENT said the Academy should be congratulated on having received such a learned paper from Dr. McWeeney. There was a case published in the *Journal of Pathology*, about three years ago, by Kanthack and Rolleston, resembling in many ways that described by Dr. McWeeney. Another case, not mentioned by Dr. McWeeney, was published in France, in 1882, of adenomata of the kidney. It was argued that adenomata of the kidney were almost invariably associated with a cirrhotic condition of the kidney. He had himself brought before this Section a somewhat similar case.

It occurred in an elderly man who was dying of cirrhosis of the kidney. There was a tumour projecting from the right kidney, about the size of a small kidney bean. It was not accompanied by metastasis. The tumour consisted of double rows of cylindrical epithelial-like cells, which contained towards their base a well-staining nucleus. The rows of cells were occasionally very like convoluted tubules. It was separated from the degenerated kidney substance by a dense fibrous capsule. There were numerous hæmorrhages and spaces lined by delicate endothelium; some contained blood, and some were empty. There was a great deal of fat. He thought it possible that the confusion which attended the subject might be accounted for by the fact that different tumours were included under one name.

Dr. McWeeney, replying, said that he had made a mistake in not looking up more thoroughly the English literature on the subject. He had, however, consulted Dr. Rolleston's Lectures on the Suprarenal Capsule, which were most exhaustive. In the class of cases the President was referring to, the tumour originated in the kidney, altogether apart from the suprarenal. In both of his cases the tumour originated in the lower end of the kidney. Because adenomata occurred in a cirrhosed kidney, it did not follow that they were caused by it. The fully formed tissue of these tumours is not like urinary tubules. In the cortex of the human suprarenal, but more especially in that of the higher mammals, pseudo-tubular structures of the same kind were found by Creighton, Marchand, Pfaundler, and others.

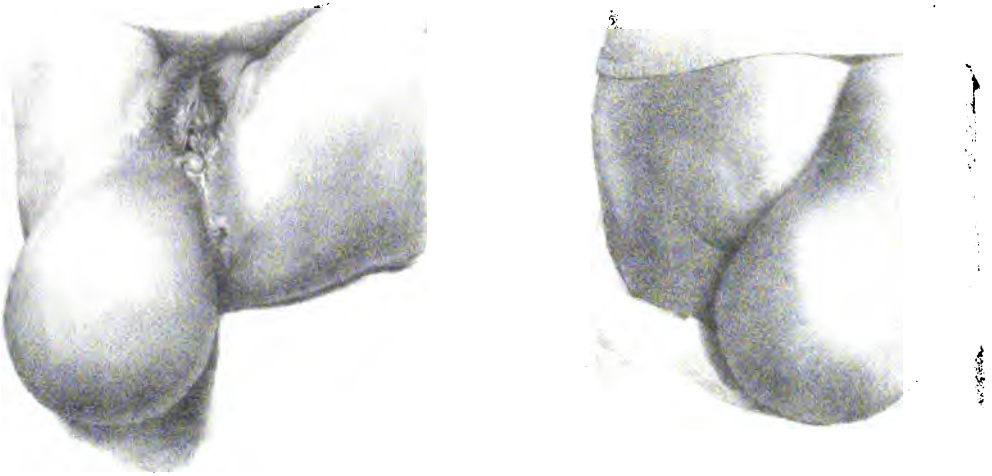
CASE OF MYXOMA OF THE BLADDER.

By CHARLES B. BALL, M. Ch., F.R.C.S.;
Regius Professor of Surgery, Univ. Dub.

[Read in the Section of Pathology, February 14, 1896.]

I am much obliged to my friend, Dr. W. J. Smyly, Master of the Rotunda Hospital, for giving me an opportunity of seeing this most remarkable case.

Case.—Margaret Tierney, aged forty-eight years, married, two children, was admitted to Sir Patrick Dun's Hospital on September 5, 1895. She stated that in July, 1881, when five months pregnant, she fell and struck her sacral region against a wooden stool; this was immediately followed by hæmorrhage from the vagina, for which she became a patient in the Rotunda Hospital. This bleeding stopped in about a week, and she was delivered of a child at full time the following November. Shortly after her confinement she had a large perineal abscess, which continued to discharge for about five or six weeks, and then completely healed. She had a second child in 1884 without trouble. From this time she remained in good health until May, 1893, when she began to suffer from retention of urine, and shortly afterwards noticed a swelling about the size of a hen's egg in her right buttock. This was quite painless, but when she coughed she felt it bulge out and become tighter; the swelling gradually increased in size; the attacks of retention of urine became more frequent; and the difficulty of passing a catheter increased. In July, 1895, she became a patient at the Rotunda, where she remained until the following September, when she was transferred to my care at Sir Patrick Dun's Hospital. On admission to the latter hospital she was found to have retention of urine, and some difficulty was experienced in passing a catheter, owing to the fact that the urethra was elongated and tortuous. On the first occasion 42 ounces of urine, pale in colour and of low specific gravity, but otherwise normal, were withdrawn. We found a very large tumour in the right gluteal region, pyriform in shape, the narrow neck in the region of the great sacro-sciatic notch,



Dr BALL ON MYXOMA OF THE BLADDER.

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while the mass of the tumour hung down over and obliterated the gluteal fold. It passed forwards in the right ischio-rectal fossa as far as the vulva, and through the vagina the mass could be felt compressing this passage laterally. At the neck it measured 20 inches in circumference, while its largest circumference was 22 inches. Distensile impulse was distinct over the entire tumour, but it was uniformly dull on percussion. The portion felt through the vagina could be pushed with the finger upwards from the pelvis, but rapidly descended again when the pressure was removed. Careful compression with an elastic bandage caused a considerable diminution in size, but anything like complete reduction was impossible, the mass which remained being quite soft and dull on percussion. The diagnosis made was of sacro-sciatic hernia with a large amount of solid matter in it; and an operation for its radical cure was attempted. An incision was made over the neck of the tumour, and layer after layer of mucoid-looking tissue, which separated easily, was divided, but without exposing any "sac." I then attempted to divide across the neck as it came out of the pelvis; it was ligatured and divided, bit by bit, and in doing so a cavity was opened containing a small quantity of fluid, and lined with a smooth membrane, but no hernial contents. This cavity was carefully sutured up and the rest of the neck divided; the tumour was then easily shelled out of the ischio-rectal fossa. It weighed over six pounds on removal, and proved on microscopical examination to be completely formed of typical myxomatous tissue. On passing a catheter soon after the operation, bloody urine was withdrawn, and a few days afterwards some extravasation took place in the wound, which became septic, and she died on the tenth day.

At the *post-mortem* examination we found that the tumour had originated in, and completely invaded, the vesical wall. It filled the pelvis, and then made its way out beneath the great sacro-sciatic ligament, and not directly through the notch, as had been suspected. Another process, about as thick as my finger, had made its way out through the obturator foramen. The mucous membrane of the bladder was smooth and polished like a serous membrane, and all the coats of the viscus appeared to be equally infiltrated with this myxomatous tissue.

It was only when bloody urine was drawn off after the operation that I began to suspect that the bladder had been injured; but not until the *post-mortem* did we realise how completely the growth had arisen from the vesical wall.

I consider it a duty to place this case on record, the more so as our diagnosis was incorrect, and our treatment unsuccessful. I am unaware of any similar record in the history of vesical tumours.

PATHOLOGICAL ASPECTS OF THE WICKLOW MURDER.

By WM. LANGFORD SYMES, M.R.C.P.

[Read in the Section of Pathology, February 14, 1896.]

Dr. Symes exhibited specimens of gunshot wounds of the skull from a case of double murder. In addition to the interest attaching to ordinary gunshot wounds, the specimens were almost unique in the fact that, although both victims were shot simultaneously, one was killed by bullets alone and the other struck by shot alone. All the evidence went to prove that only one shot was fired at these two victims. Both their heads were together in conversation at the time, but the shot was so extraordinarily disposed in the charge that all the grains of shot hit one and all the bullets hit the other. This mysterious occurrence suggested the possibility of a second shot, but the position assumed by the bodies left no room for doubt that neither could have been shot before the other. These specimens have been presented by Dr. Symes to the Museum of the Royal College of Surgeons.

The facts are as follow:—

About two miles from Kiltegan there lived on a small farm an old man of 80 years, with his wife and servant, aged 65 and 60. He owned the farm, bore a good character, and was for years crippled with chronic rheumatism and paralysis of his lower extremities, moving about on crutches. His house consisted of three apartments—a central kitchen with a bedroom opening on either side. On the morning of 16th May, 1893, a boy returning to work at 7 a.m. saw the old man with his head out of his window shouting that "the

two women are dead, the door is locked, a man with a blackened face came in in the night, fired *two shots at them*, and I saw him go out and lock the door after him; go and fetch the police." A second labourer then arrived and found the key of the door in the centre of the yard, 7½ yards from his window. He unlocked the door, went in, and found the two women dead. The old man's story was that "on going to bed the previous night, his wife went down from her room through the kitchen to see that the servant was comfortable for the night, as she was ill. He was getting into bed, and his wife took the candle with her. There was no other light in the house. He saw a man with a blackened face enter the house and fire *two shots* at the women, retreating immediately and locking the door behind him." The police arrived and searched the house. They found a *single barrel gun* freshly discharged over the kitchen fire, a *canister of powder* in his room, with a *pouch of shot* mixed *large and small*, and a box of gun-wads.

On examining the bodies about 10 a.m., I found both women dead in the servant's room. The wife was killed by *bullets*, and the servant by *grains of shot*. The wife lay on her back on the floor with head towards the door, her left hand crossed her breast, and the right lay by her side. She was dressed with her shawl, apron, and cap on. She lay at right angles to the bed containing the servant. On turning the body a pool of blood was found under the head. There were two holes in the bonnet, and three portions of bullets were found matted in her hair. There were two bullet wounds in the back of the head midway between the vertex and nape, one on each side of the occiput. The right was a jagged circular opening admitting the point of the index finger. The outer table was *depressed and circularly* fractured in four pieces, but not penetrating the skull cavity. The left wound was higher and smaller, and admitted little

finger; it was jagged and circular, and passed directly into the brain. Portions of brain were exuding with pieces of bones. The bone admitted the little finger, and was a shade larger than the surface wound. The bullet was found in the lower part of the anterior lobe of the right hemisphere of the brain, having passed from left to right. Fragments of bone were carried in by bullet. Death was instantaneous. She was shot, while standing over the servant's bed, from behind.

The servant lay in bed on her right side, and back, facing the door. On raising the head from the right shoulder there were fourteen *shot marks* on the right side of the head and neck; one a deep puncture in front of ear, from which blood had escaped in quantity, over *external carotid* and external jugular vein. This wound severed these vessels and entered the pharynx, air escaping from it. Her death was caused by *hæmorrhage* from this wound. She was shot with *grains of shot only*; and evidently while sitting up in bed looking at the wife, who was standing beside her; she fell back and bled to death. *There were two shot marks on the bones of right temple of different sizes.*

After the inquest the bodies were buried, but the case depending entirely upon the medical evidence, the bodies were exhumed and entrusted to me and Professor Lapper for re-examination. After eight days' examination the conclusions arrived at were :—

Both women were shot simultaneously.

Both their heads were together, as the wife stood beside the bed.

The wife was killed instantaneously, and the servant died from *hæmorrhage.*

Only eighteen different grains of shot hit the servant. The rest of a charge of shot was not discovered. *None hit either the bed or wall beyond.*

One charge of *two or three bullets and a little shot mixed* best accounts for the *two deaths;* but the defence was that it was absolutely impossible that all the shot could hit one, and all the bullets hit the other.

The husband was found guilty and sentenced to death, but afterwards reprieved; however, the mystery of the *shot wounds* and *bullet wounds* must for ever remain unsolved.

The CHAIRMAN said that with regard to the finding of the *grains* of shot, he thought the difficulties were not confined to country practice. In one case Dublin surgeons had, after a most careful examination, failed to find a bullet.

DR. BENNETT referred to a case brought to Baggot-street Hospital in which the combined skill of that institution was not able to discover the bullet.

STENOSIS OF TRACHEA AND BRONCHI.

By F. ALCOCK NIXON, F.R.C.S.;
Surgeon to Mercer's Hospital.

[Read in the Section of Pathology, March 13, 1896.]

W. J., aged forty, a strong, healthy-looking countryman with florid complexion, had been under treatment in Dublin for three weeks. He was told, he says, that he had syphilitic laryngitis. He had poultices applied to his throat, but, not getting any better, he returned to the country, where, during the previous eighteen months, he had been treated for syphilis by mercury and iodide of potassium. On his return from Dublin he consulted his former medical attendant, who sent him to me for further treatment of his syphilitic laryngitis. As his breathing was so difficult, he did not consider it safe for him to be at a distance from surgical aid.

He arrived by the morning train, and called at my house. I was out when he came, and on my arrival I found his dyspnœa so urgent that I thought at one time I must open his trachea. An examination of the specimen will show how fortunate it was that I did not, as I think it more than likely the man would have died in my study. I had him admitted to hospital, and asked Dr. Woods to see him. He kindly did so, and before making any laryngeal examination, at once stated that he had nothing wrong with his larynx. The grounds upon which he arrived at this opinion—which subsequent events proved to be quite an accurate one—he will explain to the meeting. He found on examining the larynx and trachea that they were healthy, as far, at least, as the seventh ring of the latter.

It was evident, therefore, that the obstruction, whatever

it was, must be below this point, and the opinion arrived at was that it was due either to a tumour, an aneurysm, or to syphilis. There were evidences of syphilis in an active stage along one of the pillars of the fauces, and so it was decided to give large doses of iodide of potassium, beginning with 10 gr. doses, increasing to 15 grs., and subsequently to 20 grs., three times a day. Of this drug he took several ounces, but without any relief. The mass round the trachea and bronchi gradually contracting, slowly strangled him, and he died seven weeks after his admission.

Only a limited autopsy was made. The right lung was much congested and solidified. The lining membrane of the trachea and bronchi was much congested. The larynx was examined from below, and was healthy, as was the upper part of the trachea. On looking into the tube from above, a transverse slit is to be seen, marking the commencement of the stenosis; which increases as it is traced downwards, until finally the interior of the trachea will barely admit a No. 4 catheter. Both bronchi are similarly narrowed by a morbid mass pressing on them also. Dr. McWeeney kindly promised to make a microscopic examination of this and report upon it.

Dr. Robert H. Woods said that, through the kindness of Mr. Nixon, he had had an opportunity of examining the patient. The unusual character of the sound emitted during respiration attracted his attention, and enabled him, without laryngeal examination, to say that the stenosis was tracheal and not laryngeal. He inspected the trachea as far down as the eighth ring without seeing the obstruction. The stethoscope showed no difference between the two sides of the chest, and the inference therefore was that the stenosis was situated above the tracheal bifurcation and below the ninth ring. To explain the cause, they had to choose between tumour, aneurysm, and tertiary syphilis. The first two were excluded on account of the absence of other evidence in their favour; such as pressure symptoms, dysphagia, external tumour, pulsation,

auscultatory phenomena, and the like, so that, partly by a process of exclusion and partly from the positive evidence of the history of the case and the presence of active syphilis in the throat, they concluded that the stenosis was caused by gummatous deposit round the tube, and the specimen showed this view to have been correct. The question of surgical interference was discussed, but was thought impracticable, and a look at the specimen now was sufficient to show that had it been attempted death would, in all probability, have been precipitated, and could not have been averted.

Dr. McWeeney said the tumour consisted of lymphatic gland tissue, the central portion of which had undergone caseous degeneration. There was also a considerable amount of fibrous tissue and giant cells. He considered that it was a lymphatic gland which had been the seat of gummatous deposit.

Mr. Croly, some years ago, had a patient with syphilitic laryngitis. He suddenly developed symptoms of asphyxia. He had him brought to hospital and performed low tracheotomy on him. When he put in the tube no air came through it. He pulled it out and put his little finger into the trachea, and found it filled with soft granulation tissue. He made an opening through it with his finger and inserted the tube. The patient lived. In Dr. Nixon's case he would have performed tracheotomy, and tried to put in a long tube. If the right and left bronchus were at all pervious he would then have had a chance.

Mr. Nixon, replying, said that the question of tracheotomy had been considered over and over again. The diagnosis was extremely doubtful. They were not sure whether they were dealing with an aneurysm, new growth, or intra-tracheal tumour. He thought tracheotomy would have hastened the man's death. The diseased mass overlaid not only the trachea but also the right and left bronchi. The lumen of the tube was less than would admit a No. 4 catheter.

A CASE OF SUDDEN DEATH DUE TO THROMBOSIS AND ADVANCED ENDARTERITIS OF THE CORONARY ARTERIES.

By J. MAGEE FINNY, M.D. Dub.;

Ex-President, Royal College of Physicians in Ireland;
King's Professor of Practice of Medicine in the School of Physic in Ireland.

[Read in the Section of Pathology, March 13, 1896.]

THE following are the clinical notes, for which, and for the record of the *post-mortem* examination, I am indebted to Mr. Moorhead, Medical Resident Pupil:—

CASE.—H. M., aged fifty-eight, was admitted to Sir Patrick Dun's Hospital on November 5th, 1895, complaining of pains in the front of the chest, and in the back between the shoulders; also of severe attacks of breathlessness on the least exertion, and of palpitations which were extremely distressing, and generally came on at night. She has had a cough and muco-purulent expectoration for some months, and feels extremely weak.

Previous Clinical History.—Patient had an attack of scarlet fever when a child. She also had two attacks of what she terms "fever," accompanied by rheumatic pains; neither attack lasted more than three weeks. The first attack occurred in 1882, and the second about 1889. She has borne several children, and led a hard life. Of late her strength has been failing. The pains in the chest of which she at present complains began about three weeks previous to admission.

Present Condition.—A pale, wasted, worn-looking woman, with flabby muscles and sallow skin. The feet and legs are swollen and œdematous, and are covered with small purpuric spots, which are also scattered irregularly over the trunk and upper limbs.

Temperature on admission, 99·5° F.; respirations, 34; pulse, 90, extremely irregular, and at times intermittent; arteries tortuous and atheromatous; urine acid, sp. gr. 1015, contains a trace of albumen; no blood. There is evident dyspnœa, amounting at

times to orthopnœa. The chest is resonant on percussion; râles and rhonchi are audible all over the chest, in front and behind. The apex beat of the heart is displaced downwards and to the left, the maximum impulse being in the 6th interspace; the impulse is diffused; area of cardiac dulness increased downwards and slightly outwards.

On auscultation a somewhat harsh, loud, and prolonged blowing murmur is heard, with and partially replacing the first sound in the initial area. The murmur is audible in the axilla and behind, just internal to the angle of the scapula. The pulmonary second sound is accentuated. The area of liver dulness is increased downwards between two and three fingers' breadth below the ribs.

Nov. 10th.—Patient gets sudden attacks of great dyspnœa and orthopnœa which occur chiefly at night. The urine contains more albumen, sp. gr. 1020, colour normal. The œdema of legs is greatly diminished, and the purpura is less marked.

Nov. 13th.—Urine sp. gr. 1020; contains blood. Microscopically it contains granular and blood casts, epithelial and blood cells, and granular *débris*. Dyspnœa worse to-night; pulse, 112; irregular respirations, 36; temperature, 98°; slight dulness on percussion at both bases behind; rest of chest resonant; breath sounds normal, but somewhat weak; there is no bronchial breathing or bronchophony; râles and rhonchi diminished; expectoration not abundant, somewhat viscid, muco-purulent, of a greenish yellow colour, not rusty.

Nov. 17th.—On the evening of the 17th the dulness over both bases behind has increased. Breath extremely weak over both bases, and almost inaudible over right base from the level of a horizontal line three fingers' breadth below angle of scapula downwards. The pulse is weak and very irregular. Patient can breathe only in the upright position; respirations, 40. Patient complains of great pain in the cardiac region, radiating to the left and backwards, and looks extremely weak still.

Nov. 18th.—8 30 a.m.—The condition of patient remains unchanged for the better. She has had a bad and restless night, with much orthopnœa. Pulse, 108, weak and irregular; temperature, 96° F.; respirations, 40.

10 30 a.m.—Patient died suddenly. The nurse had been speaking to the patient, and turning away crossed to the opposite end of the ward. Hearing a sound in the bed she looked back. The patient's eyes were fixed and staring; the shadow of

death crossed the face, and the jaw had dropped. Dr. Finny was summoned from the next ward, and saw her immediately, but the heart sounds could not be heard on auscultation, and there was no pulse at the wrist. Strong ammonia to the nostrils and the hypodermic injection of ether produced no response.

The *post-mortem* examination was made on November 19th. On opening the chest a considerable quantity of clear fluid escaped from each pleural cavity. The lungs were small, normal in colour; consistence normal everywhere, except at both bases, which were slightly firmer. On section, bases of lungs somewhat engorged with blood, a good deal being squeezed out by pressure; no pneumonia; pieces of lung float in water; no pleurisy. A few drachms of fluid in pericardium; heart hypertropied on left side. On section, fat lay in the sulci, and the ventricles were infiltrated with fat, and there seems to be, from the pale colour, a good deal of fatty degeneration, most marked in right ventricles. Mitral valves somewhat calcareous and covered with vegetations, which have also become hard and calcareous. The aortic valves are thick and covered with rough granulations on the ventricular aspect, but seem to close the orifice. Aorta and coronary arteries show atheromatous patches, and the coronary arteries, especially the left, are brittle and calcareous. The left sinus of Valsalva and the beginning of the left coronary artery are occupied by a blood clot which was traced down for 1–2 inches. There was no clot in the aorta or ventricle, or right coronary. The right side of the heart was not distended with blood. Liver enlarged, heavy and engorged with blood. On section, presents appearance of nutmeg liver. Spleen normal. Kidneys small and firm; capsule adherent. On section, intensely engorged with blood.

The microscopic examination and sections were made by Dr. Charles Maunsell (our House Surgeon):—

Liver.—Well-marked cyanotic atrophy.

Kidneys.—Engorgement of chronically diseased organs—(1) some thickening of capsule; (2) irregularity of epithelium lining tubes; (3) thickening of arteries in places; (4) atrophy of many of the glomeruli, which are replaced by nodules of fibrous tissue; (5) small-celled infiltration in many parts.

Heart.—(1) Some indistinctness of striation; (2) in the vicinity of many branches of the coronary arteries the fibres are broken down, and there is much fibrous tissue and irregular detritus;

(3) groups of refractive globules are to be seen in the body of muscle fibres; (4) on treating fresh section with osmic acid these globules stain black.

Remarks.—The *rationale* of the case and the course of the morbid changes seem to me to be as follows:—First of all, chronic arterial and valvular disease, owing to end-arteritis and valvulitis, existed. These were, in all probability, produced by the attacks of fever and "rheumatic pains" in 1882 and 1889, and by the hard life the patient lived, and by the number of pregnancies she had gone through.

The intra-thoracic pains, of which she complained for three weeks before admission to hospital on Nov. 5, 1895, and which were accentuated the day before her death, were due to the impaired circulation in the aorta and in the coronary vessels, which were undergoing atheromatous and calcareous changes with diminution of their calibre; and as the result of impaired nutrition through these vessels, the muscle of the heart was becoming fibrous, and fattily degenerated. The nocturnal dyspnœa and the palpitations were attributable to these changes being chiefly in the muscles of the right ventricular wall, and thereby failing compensation of the mitral regurgitant disease was induced.

The enlarged nutmeg liver, the pulmonary venous stasis, and the passive double hydrothorax were, in like manner, due to compensatory failure.

The renal hæmorrhages—very unusual in cardiac disease, and then due in the vast majority to embolism—were, in this case, due to the atheroma of the minute branches of the arteries and to their rupture, of which a good specimen is to be seen under the microscope. In all probability the purpura of the upper and lower limbs—the pathology of which condition is still shrouded in mystery—had its

factor in the same arterial changes in the cutaneous capillaries, being accentuated by the altered blood crasis.

The chief interest of the case is, however, centred in the production of the sudden death of the patient. For, ill as she was for a day or two before the morning on which it occurred, there were no special warnings of its approach. I do not think the fatty degeneration of the heart of itself to be a sufficient cause in this case, as one meets with many cases in which there was far more extensive muscular degeneration and infiltration than in this, and yet death may not be due to it, and is not sudden except rupture occur. I attribute it rather to a paralysis of the heart, due to an acute thrombosis of the left and anterior coronary artery, which produced sudden and permanent anæmia in an organ already heavily handicapped, and an inhibitory action through its nerves.

Sudden death is not a very unusual termination of disease of the aortic valves producing great regurgitation. It is also found in cases of aortic aneurysm, even although the sac may not have given way, and in cases of advanced fatty disease of the heart. Moreover, where that organ is widely dilated and much weakened in long-standing mitral disease or renal cirrhosis, sudden death may close the scene.

One must never forget the important part the coronary vessels play in two at least of the above cases, and the subject of my communication illustrates how disease of these vessels alone may prove another cause of sudden death. The anatomical characteristics of the coronary arteries help to explain the occurrence of sudden death in case of their occlusion. Their terminal branches are end arteries, and they arise in the sinus of Valsalva. Thus their origin forms a part of the aorta, where atheromatous disease is a very common condition, and which is so often

associated with aortic regurgitant disease in elderly people, and it is in such cases that death may occur, as anæmia of the ventricles is probably the cause of their sudden failure to carry on the circulation.

According to Osler,* embolism or thrombosis of one of the coronary vessels will lead to a condition which is known as *anæmic necrosis*, or white infarct—which is most commonly seen in the left ventricle and in the septum—in the territory of the distribution of the anterior coronary artery. The affected area has a yellowish-white colour, sometimes a turbid, parboiled aspect, at others a grayish-red tint. Microscopically the changes are characteristic. The nuclei disappear from the muscle-fibres, a condition of fragmentation is present, and the fibres present a homogeneous, hyaline appearance.

Sudden death, Osler states, not infrequently follows the blocking of one of the coronary arteries and the production of this anæmic necrosis, and he calls attention to the importance of such an occurrence in a medico-legal point of view, as it may be the sole lesion in some such cases of sudden death.

My case illustrates the paralysing effect of sudden thrombosis of a large vessel, where the ventricles were already in the pathological condition of fibroid and fatty degeneration and the arteries were the seat of extreme endarteritis and sclerosis.

Since writing this paper my attention was called to a valuable article on the intimate relation between Fibroid Degeneration of the Heart and Coronary Disease, by John Lindsay Steven, M.D., in the *Journal of Pathology and Bacteriology*, November, 1893.

* Principles and Practice of Medicine, p. 675. Osler. Second Edition.

Dr. McWeeney said that the subject was a highly important one, in view of the difficulty sometimes encountered in arriving at a conclusion as to the cause of sudden death. Atheroma of the coronaries should always be carefully sought for. In his experience death was not due directly to this cause but to the small patches of fibrous tissue, the so-called Herz-schwielen of the Germans. These were the almost invariable result of atheroma of the coronaries, and would account for death independently of either valvular disease or thrombosis.

Dr. O'Carroll had seen two cases bearing on the subject of sudden death. One was a case of aneurysm of the apex of the heart. The patient lived forty-eight hours after being conscious of a sudden pang. The pain lasted till the end. *Post mortem*, it was found that the apex of the heart bulged out to the size of a walnut. The other case was that of a young man who died absolutely suddenly in a lodging-house in Church-street. The mitral valves were covered with large cauliflower granulations, about the size of a pea, but oval in shape. One of these oval nodules had been dislodged and sent lengthwise into the posterior coronary artery. It was interesting to contrast the two cases. The higher in the coronary artery the plug occurs the more sudden the death. Further down more likely to have aneurysm, and further down still patchy degeneration.

Dr. McWeeney said that atheroma of the coronary arteries might very easily escape notice unless looked for. Dr. Finny's paper would attract attention to it. He had seen several cases of it, especially when on duty at the Morgue. Death could not be ascribed directly to the coronary arteries, but to the muscle of the heart whose blood supply was affected. Foci of fibrous tissue were scattered through the heart in these cases. In the first section under the microscope the heart muscle was becoming fibrous. It was the first stage of a cicatrix. In the second specimen the granules did not appear to him to be fat. He thought it was a good example of brown atrophy.

Dr. Conolly Norman had seen the first case referred to by Dr. O'Carroll. He had not mentioned that the aneurysmal dilatation of the heart had ruptured. He had seen another case of an old woman who had symptoms which suggested fatty heart. She died suddenly, and the anterior branch of the left coronary artery was found absolutely occluded for two inches by endarteritis. The patch of the ventricular wall supplied by it was very thin and pale,

and presented the microscopic appearances described by Drs. Finny and McWeeney.

Dr. Finny, replying, said that in Dr. O'Carroll's second case there was a distinct embolus. In his own case an already weak heart suddenly ceased from a thrombus forming. In consequence of the contraction of the smaller branches of the coronary arteries anæmic necrosis may occur which ends in fibrous tissue. The refractive globules stained black with osmic acid.

ON A LARGE MALIGNANT GROWTH OF THE RECTUM, WITH ABNORMALITY OF THE KIDNEYS.

By W. I. WHEELER, M.D., F.R.C.S.;
Surgeon to the City of Dublin Hospital.

[Read in the Section of Pathology, April 24, 1896.]

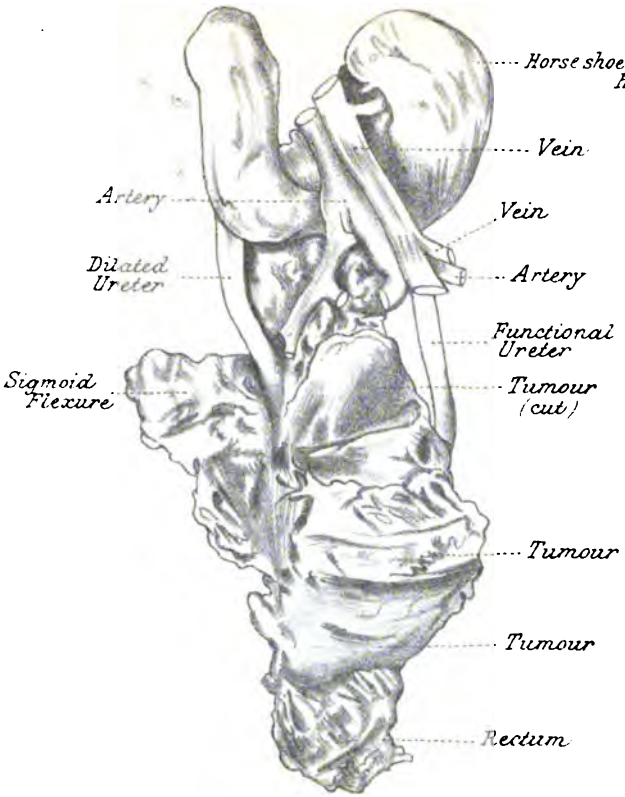
The subject from whom this specimen was removed was a man, thirty-eight years of age, who was admitted to the City of Dublin Hospital, in September, last year, and whose history, as he related it, was as follows:—

He stated that he was well in health up to April last, that is, April, 1895; and then his bowels became constipated, since then only being moved when he took medicine. He complained of constant pain across the lower part of his stomach, which was more acute at night. Upon palpation a large hard tumour could be felt, almost filling up the left iliac region, and extending upwards for a considerable distance above the umbilicus. By digital examination the tumour could be felt in the rectum about 4 inches from the anus, and by external palpation, together with rectal examination, the tumour seemed to be movable. He had lost weight and was of sallow complexion. Upon the night of his admission he had a motion consisting of mucus, stained with bile; he passed a large amount of urine; there was not any diminution in the kidney secretion, which was normal, until ten hours before death. Since his admission his bowels had always to be relieved by medicine, the motions being usually formed but flattened; he experienced much and increased pain when about to evacuate.

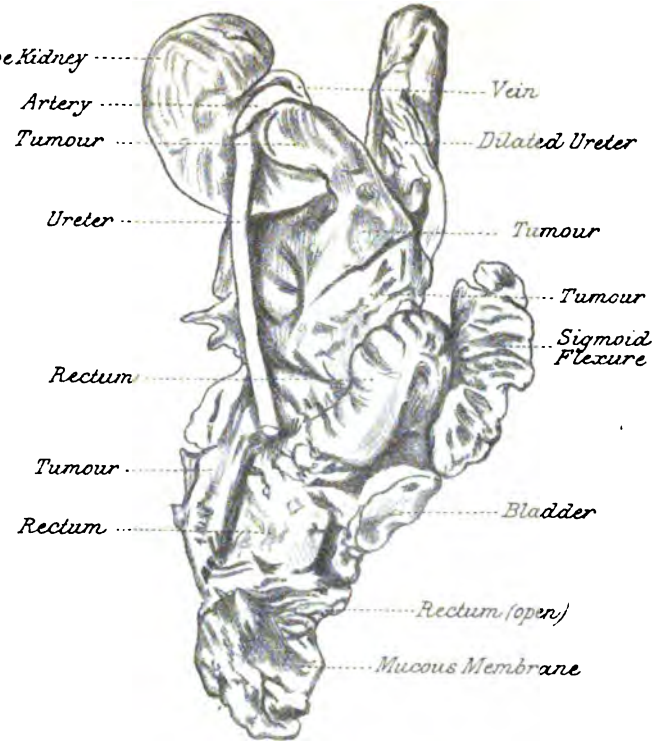
An exploratory incision about 3 inches in length was made in December last, when it was ascertained that the tumour could not possibly be removed by operation. Eleven days after he died, apparently from asthenia. The urine, as stated above, became scanty.

Autopsy showed that the body was slightly emaciated. An incision about three inches long extending downwards from the

BACK VIEW.



FRONT VIEW.



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Mr WHEELER. — MALIGNANT GROWTH OF RECTUM. ABNORMAL KIDNEYS.

umbilicus was healed. On cutting through this the muscles were found to be united. On reaching the peritoneum some increased vascularity could be observed. Upon opening the peritoneum a loop of intestine, immediately under the incision, was found to have an inflammatory stain on it, about a quarter of an inch in length. The intestines were found matted together by old adhesions and difficult to separate. Upon cutting through the mesentery a large tumour was seen extending from the sacro-coccygeal articulation to above the second lumbar vertebra, and pressing upon the left kidney, which had been converted into a cyst, and also pressing upon the lower part of the right kidney. The tumour was hard and nodular. The glands in the mesentery were small but hard; the tumour was very firmly adherent to the sacrum, and on being separated upwards the bone was found denuded and roughened. The growth extended into each iliac fossa, and was beneath the iliac fascia. Superiorly could be seen the aorta and inferior vena cava, surrounded by, and almost disappearing in, the tumour. The small intestines were distended with gas; there was not any trace of recent inflammatory action, except the small stain already alluded to. The cæcum was distended with flatus; in the ascending, transverse, and descending colon could be felt small masses of hardened fæces. The sigmoid flexure was pushed over to the right side of the middle line, and externally could be seen masses of hard, white growth, not, however, involving the lumen of that portion of the gut; the upper part of the rectum was completely involved and surrounded by the growth, and from the posterior part of the mucous surface of the rectum the growth appeared to have had its origin. The posterior wall of the bladder was adherent to the tumour. The left ureter was embedded and nipped by the tumour and dilated; the right ureter was normal. The stomach and spleen were healthy, also the liver, except on the right edge of the right lobe there was a hard white mass about the size of a bean.

Doctor McWeeney, who kindly examined the tumour, writes:—"I have sectioned the pieces of rectal tumour and find it to be a case of cancer."

The kidneys presented the abnormality called horse-shoe. The two halves were complete and perfect in themselves, having a distinct pelvis and ureter. The connecting portion of isthmus was merely composed of condensed fibrous tissue, and was not a secreting structure; the ureters descended in front of the transverse or connecting portion, and crossed each other on their way to the bladder.

The left kidney, as previously mentioned, had been converted into a mere cyst from pressure of the growth. The arteries were normal.

Professor Macalister, of Cambridge, has written an interesting account of the irregularity of the renal arteries in the *Journal of Anatomy and Physiology*, 1883, classifying them under Varieties of Numbers; Varieties of Origin; Varieties of Entrance into the Kidney; Varieties of Distribution of Branches; Anomalies of Branching and the Multiplication of Renal Arteries.

In Reynolds' "System of Medicine," Vol. V., page 647, an account of the anomalies of kidneys will be found—both anomalies of form, structure, and development, also a description of supernumerary, solitary and horse-shoe kidneys. A number of cases of absence of both kidneys have been described by Rayer.

In the *Dublin Quarterly Journal*, 1866, Dr. MacNaughton Jones describes an abnormal kidney. The aorta had curved underneath the kidney, the iliac arteries deviating from their usual course. The kidney is described as the form alluded to by Rokitansky.

In the *British Medical Journal*, Vol. II., 1870, is described a fatal injury to the kidney in a subject possessing only one kidney.

Mosler and Rayer have recorded a number of solitary kidneys. The left kidney is more frequently absent than the right. This defect may be congenital or acquired.

Renal abnormality is described in *British Medical Journal*, 1882, and in the Transactions of the Pathological Society of London, 1851, Vol. III.; of 1855, Vol. VI.; of 1862, Vol. XIII.; of 1866, Vol. XVII.; of 1868, Vol. XIX.; of 1869, Vol. XX.; of 1872, Vol. XXIII.; also in the *British Medical Journal*, 1883, Vol. II.; 1886, Vol. II., page 159; *Dublin*

Journal of Medical Science, Vol. XVII., page 391; the *Lancet*, 1885, Vol. I., page 979.

The writer has seen a large calculus embedded in the connecting bond or isthmus of a horse-shoe kidney.

The original photographs were kindly taken by Dr. Fraser, Professor of Anatomy, Royal College of Surgeons.

Dr. McWeeney had received part of the tumour for microscopical examination. It was a carcinoma, but could not exactly be called either scirrhus or encephaloid. It had not sufficient fibrous tissue in proportion to the glandular tissue to be called scirrhus. The shape of the cells was not readily determinable, as the specimen was not in good condition. With reference to the horse-shoe kidney, he wished to know whether both kidneys were displaced downwards and over what vertebræ the connecting band between them passed, and the relations of the ureters?

Mr. Wheeler, replying, said that the ureters were in exactly the normal condition. The kidneys were a little lower down than usual, and the connecting band passed over the second lumbar vertebra.

CHRONIC RHEUMATIC BURSITIS, INDEPENDENT OF THIS DISEASE IN ADJOINING JOINTS.

By E. H. BENNETT,
Surgeon to Sir Patrick Dun's Hospital;
Professor of Surgery, Trinity College, Dublin.

[Read in the Section of Pathology, May 29, 1896.]

In 1882 I presented to the Pathological Society an example of bursal disease, similar in its pathological detail to chronic rheumatic arthritis of joints, but occurring independently of any direct connection with a diseased joint.

Mr. Adams heads his chapter on "Bursal Tumours" thus—"Bursal Tumours developed in the vicinity of the Joints affected with Chronic Rheumatic Arthritis," and throughout his discussion he adopts the view that "such bursal tumours may be considered symptomatic of this disease existing in the neighbouring joint." The specimen I now submit is almost identical with the example I exhibited in 1882, and, like it, presents well-marked chronic rheumatic bursitis, occurring independently of disease of the neighbouring joint. This specimen shows the bursæ between the tendons of the obturator internus and the pully-like surfaces of the lesser sciatic notches diseased. The cavities run into the bursæ beneath the attachments of the hamstrings to the tuber ischii on each side, and the surface of all the involved bone is roughened, and gives to the touch the rugged feel characteristic of bone undergoing absorption.

The tendons involved show a condition of fraying similar to that so constantly seen on the surface of the biceps tendon in the shoulder-joint affected by chronic rheumatic arthritis. In this specimen we have the hip-joint preserved on the left

side, and it is evident that it has not suffered to any marked degree from chronic rheumatic arthritis. And from the condition of the opposite acetabulum we can state the same for that hip.

The symmetry of the bursal disease constitutes a very remarkable feature, and is sufficient to place the case in the category of the constitutional form of the disease as defined by Adams.

The great trochanter exhibits also well-marked chronic rheumatic bursitis, similar to that involving the lesser sciatic notches and the attachments of the hamstrings. I regret that I had not the opportunity of examining the great trochanter of the opposite side, as it had been removed some time before my attention was directed to the dissection.

The portion of the spine connected with this pelvis shows that chronic rheumatic arthritis had established itself in the articulations of bodies of the vertebræ, for there is a well-marked development of the osteophytes which grow in the anterior common ligaments under its influence.

I have seen this form of bursal disease pretty often during dissection, and I have been able, in a few instances, to recognise it in the living. Last year I saw a specimen in all respects similar to this, and I would have gladly preserved it, but it was prepared for a special anatomical demonstration, which prevented my obtaining it. This season I published in the anatomical department my want of such a specimen, and in a very few weeks it was discovered for me. I mention these facts to prove that chronic rheumatic bursitis is not an uncommon disease. I have recognised it in the living in some of the superficial bursæ, but as yet I have not met with it in the lesser sciatic notch or tuber ischii.

Certain it is that one suffering from this affection, would, should he drive on our roads, need the support of pneumatic tyres. The fact that in these bursal sacs there are no carti-

lages present, must modify the view so often expressed that a histological change in cartilage is the essential feature of the disease. It can develop in situations where no cartilage exists.

The following passage, a translation from Cornil and Ranvier, states the opinion commonly held as to the essential character of these pathological changes:—

"These arthritics are essentially characterised by a villous state of the cartilages, by a hypertrophy of the synovial fringes, by ecchondroses or osteophytes around the cartilaginous investment. Whatever the joints attacked, and in all the forms of the malady, the histological process is the same. It consists essentially in a proliferation of the ancient cartilage, and in a new growth of cartilage on the fibrous parts, from which it results that the term which defines it best is 'arthrite proliféranse' ('the velvety state' of Redfern, Weber, Volkmann)."

CHRONIC RHEUMATIC ARTHRITIS IN THE ARTICULATION OF THE SKULL WITH THE SPINE IN THE RORQUAL, BALÆNOPTERA ROSTRATA.

By E. H. BENNETT,
Surgeon to Sir Patrick Dun's Hospital;
Professor of Surgery, Trinity College, Dublin.

[Read in the Section of Pathology, May 29, 1896.]

The occurrence of chronic rheumatic arthritis in members of the class mammalia other than man has been long known. I have seen it in the monkey, the bear, the hyæna, the dog, the horse. That the disease is respectable in age is proved by its presenting itself in the articulations of the ancient Egyptian mummy, but even older in cervus megaceros, the great fossil elk of Ireland. The latest addition to this category is that which I now submit to the Academy—an example of well-marked chronic rheumatic arthritis affecting the joint connecting the head and the first cervical vertebra in the rorqual, balænoptera rostrata.

I am indebted to Dr. Dixon for the opportunity of showing this specimen, part of which he has kindly presented to the Pathological Museum of Trinity College.

He found the whale stranded on the shore of Achill, and himself, at great labour, procured and prepared the skeleton.

This fact is of special interest, as some sceptic might, if we had no history, object to the diagnosis, and attribute the eburnation and polish of these articular surfaces to some cause acting *post mortem* on this skeleton. One might ask could the rocking of the dead skeleton in the sand with the rise and fall of the tide and waves have produced it. The flesh of the animal had to be cut, and the ligaments of the

joints divided, so no sand had access to the joint cavities. I raised this question myself, because I am aware that a high degree of polish is seen on bones from causes other than chronic rheumatic arthritis—for instance, in joints of man affected with suppuration it happens that necrosis of a large part of the articular surface may take place. When this occurs the soft portion of the cartilage becomes macerated away in the fluid collected in the joint, and the calcified layer becomes exposed, and readily takes a polish under the friction of very limited range, which must take place in any such joint. I present two specimens, one from a human knee-joint, and one from the lower end of the humerus, which show the polish so developed in suppurative arthritis of the joints.

There is, again, a polish seen to be developed on the bones of the dead skeleton under circumstances which occurred to me as possible in the case of this whale, but which the fact of Dr. Dixon's dissection excludes.

In many bones of megaceros found in our bogs this polish has been observed where the shafts of the bones lay across each other in the turf. These are examples, and they show that a very high degree of polish is developed where two osseous surfaces lie in contact, with only such minute movements on each other as the vibrations of the bog permit and induce.

I think then, by way of exclusion, we must arrive at the diagnosis of chronic rheumatic arthritis as having been pretty well developed in this whale's occipito-atlantoid joints.

SECTION OF STATE MEDICINE.

FIRST PRINCIPLES IN SMALL MATTERS OF PREVENTIVE MEDICINE.

By WALTER BERNARD, F.R.C.P.;
Consulting Physician to the Hospital for Contagious Diseases, Londonderry.

[Read in the Section of State Medicine, April 17, 1896.]

WE who can so fully realise that State Medicine rests on a foundation of first principles and prevention, cannot but deplore that its ascertained hygienic rules have not obtained more special recognition from administrators, legislators, and municipal bodies in ordinary communities—communities which frequently hinder rather than aid social progress.

On so important a subject, which concerns the health of millions in human habitations, this Section will, I am sure, pardon me for intruding on their patience for a few minutes in regard to the minor details of prevention, which can be easily carried out in the homes of the people.

Amid the shifting scenes of the family physician's daily life, while he is unrolling the panorama of human miseries and sufferings, the conviction is daily gaining more and more ground that he possesses the greatest facilities for the application of principles in domestic hygiene—facilities which enable him to get at the antecedents, and the causes calculated to destroy health, embitter life, and, on the whole, rob the nation of that which ought to belong to it. When the family doctor possesses the courage of his con-

victions in sanitary domestic measures, and is not afraid of the landlord or tenant, he cannot be too highly appreciated by the public; and though he may smart and wince for being independent, yet he will soon rise above the level of all that is acrid, unjust, and selfish.

If we have the courage of our convictions how frequently we must deprecate, to the minor insurance agents and to the people, the subterfuge and intrigue practised by their burial societies. Medical men are frequently asked to certify that an illness had lasted for a short period, when it was well known that the deceased had been ill for two or three years previously. Who is to blame? Certainly the State. Here Shakespeare analyses our Christian example: The deception "you teach me I will execute, and it shall go hard but I will better the instruction."

As we go on we are learning, year by year, more of what lies before us. During our daily visits we are not too busy to spend a few minutes on controlling influences. A mutual good understanding will, without giving rise to anything calculated to cause offence, enable us to inculcate and exercise hygienic influences on the minds of men, women, and children. And we give proof to the household, while we thus uphold preventive rules, that we are dealing only with life regulated by laws calculated to remove from the family all the removable causes of contagion, or other factors which bring death to one or more of its members. In this way we are continually afforded opportunities of inculcating in the minds of a law-abiding people that it is the duty of every individual not only to protect himself and family, but to assist in investigating and staying the spread of contagion. The absence of the extension of sanitary scientific knowledge, the crime of concealment, and careless indifference, have permitted

scarlet fever to spread over Londonderry and its neighbourhood. Some of the children were allowed to play on the streets during the process of desquamation, and some of their near relations did not use means to free themselves from contagion before going into crowded assemblies and railway carriages. These facts, among many others, go to show that notification is not fully enforced.

It is a matter of every-day experience that if isolation and disinfection be faithfully and energetically carried out, the further spread of contagion can, as a rule, be put a stop to. This is a most important fact for both employers and employed, and is well worth attention. For, if our efforts be successful, and isolation be effectively carried out, not only do we save life, and frequently protect the young from the sequels of such diseases, but we can even allow the bread-winners to attend their work.

Under the boarding-out system in Gwyn's and Young's Trust, I have succeeded, with one room only at my disposal, in checking the spread of the epidemic to other members of the family and to lodgers. I exhibit models and illustrate apparatus I employ. Articles can be rendered aseptic by heat and antiseptics before leaving the sick room. One is adapted for overalls worn by those entering and leaving the apartment, and is specially contrived for the doctors' and nurses' caps and gowns. But unless the medical man himself reminds the persons in immediate attendance on the sick of all that has to be rendered aseptic, such as the anointing of the body and the scalp, as well as the burning of hair, nail clippings, old neckties, slippers, tooth-brushes, our efforts will be probably unsuccessful. If we succeed, we keep the contagion from our own children as well as from those of our patients, despite the damp basements and the paved or unpaved yards. Should our efforts be unavailable, we shall have

the satisfaction of knowing that no act or any neglect of ours omitted to shut the gates against that which brought calamity to the household.

Local prejudices and the love of money and power still stand much in the way of social progress in Londonderry. However, against these hindrances we can show excellent results, and what has added much to our success is that, at the suggestion of your Health Society, I was enabled to establish a branch in our city, and the Duchess of Abercorn graciously consented to become President of it. Our progress has been exceptionally good, and, in a great degree, is due to our Secretary, Mrs. Reed, of Duncreggan, whose untiring energy, keen insight, and social influence have not only gained excellent audiences for the lady lecturers, but have enabled us to extend our boundaries to Coleraine, Ballymoney, and Strabane.

There is another important step to be taken yet. The local authorities, who are compelled to find proper food and lodging for the "psychological" tramp, are not compelled to look after the housing of the honest working man. Some of the city refuse is disposed of on the catchment area of our water-supply, and some on polluted subsoil for elevation. On this houses are being continually built without concrete foundations. In the low-lying level, the families who live in properly-constructed houses, with concrete foundations (plans of which I beg to submit for your inspection), have better health than their neighbours who do not enjoy the same privileges.

As the health of the community is of more importance to the nation than fleets of ironclads, and sanitary work is spreading throughout Ireland, an International Congress of Hygiene held in Dublin would tend to awaken the public and the authorities to the vital necessity that exists for more effective measures, without which the United

Kingdom can never hope to continue to be the greatest power among the nations of the earth.

Another branch which concerns the general health is one in relation to which the general practitioner cannot be accused of over-plainness of speech, as it is a delicate subject for him to touch upon. But the personal and family history of three generations, the more accurate diagnosis, and successful methods of treatment—all tend to show that we cannot go on closing our eyes to this great evil of wide-spread specific disease. The innocent suffer because the State will not interfere. From the knowledge of human life acquired from the different phases of character during many years of practice, I am convinced that the general medical practitioner has more influence and knowledge in the physiological moulding of defective infantile life, than the hospital physicians, clinical teachers, or officials. Of necessity, his daily work among this class is more wide-spread, and brings him constantly in touch with the infant as well as the mother.

My long experience has impressed me so much with these facts of national concern, deduced from positive reiterated evidence, that I have frequently advocated my views from time to time. Were the State to recognise and realise more fully the practitioner's *locus standi*, and thus co-operate with him in his efforts, his authority in the homes would be more felt when supplemented with the stamp of State measures.

I see by the *Lancet* that a Society was formed a few months ago for instructing parents and teachers how to deal with the defective. But no allusion is made to the family doctor, whose calling brings him into immediate contact with child-life long before the scholastic teaching is reached.

Dr. Edgar Flinn asked whether the Infectious Diseases Act was enforced in Londonderry. If it was there must be a great defect in the urban sanitary authorities when the children were allowed to run about the streets in a state of desquamation after scarlatina.

Dr. Doyle wanted to know the disinfectants Dr. Bernard used, and how long the process lasted.

Dr. Bewley asked whether a separate apparatus was supplied to each family where there was scarlatina.

Dr. Bernard, replying, said the Infectious Diseases Act was only partially enforced. The antiseptic he used was eucalyptus oil and carbolic acid. This was heated, and the articles were fumigated for a considerable time. The apparatus was 6 feet long and 13 inches wide, and could be carried from one house to another after being rendered aseptic.

NOTES ON PARKNASILLA AS A HEALTH-RESORT.

By D. EDGAR FLINN, D.P.H., F.R.C.S., M.R.C.P.;
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[Read in the Section of State Medicine, April 17, 1896.]

THERE are few wants more sensibly felt in this country than that of a suitable health-resort to which, without leaving their own country, persons suffering from chronic forms of disease, or convalescing from some acute form of illness, may have recourse without undue expense or need of long travel; and my object in this short communication to this Section is to draw attention to a hitherto comparatively unfrequented district in the South-west of Ireland which has been lavishly endowed by nature, and possessing most of those advantages which make a health-resort salubrious and attractive.

Where is Parknasilla? is a question that will naturally arise in the form of inquiry by many. Parknasilla is situated on the northern shore of Kenmare Bay—a bay rich in beauty, and with singularly indented coast lines. Its well-sheltered position amidst a number of islets, thickly wooded down to the water's edge, has endowed it with unique advantages. This protective area gives to Parknasilla claims of a special character, and prevents the access to it of all winds except those coming from the warmer points—viz., South, and South-west, these winds before reaching the southern coast of Ireland having travelled over the Gulf Stream, and being thus subjected to its moderating and balmy influence.

We all recognise what elevation of the land will do for any place, particularly if it shelters that place from winds blowing

from the cold quarters. Thus mountain protection is of supreme importance in the choice of a health-resort, more especially in the winter and spring seasons of the year. In this regard Parknasilla is exceptionally favoured, a mountainous range closely guarding and protecting it from the northerly and easterly winds. The combination of mountain, wood, and water, gives a special charm to this locality; and a convincing evidence of the mildness of the winter and early spring here is the forward character of the vegetation, the early budding of the trees, shrubs, and flowers—all bearing testimony to the mildness of the climate. Temperature rapidly tells its tale on the vegetable world, and there can be no more reassuring proof of the equable and balmy character of the climate of a district than the early growth of flowering shrubs, plants, and table produce.

The position of this favored and sheltered sea inlet upon the isothermal map shows it to have a mean annual temperature of 52°, being similar in this regard to its neighbour, Glengarriff, and registering a higher mean annual temperature than Ventnor or Torquay. The mildness of the climate in the earlier spring months is of such a character that exercise can be freely partaken of in the open air daily, without risk of chill, and this to the invalid is of paramount importance. No record has, as yet, been regularly taken of the daily sunshine, or of the rainfall, but so far as could be ascertained, the rainfall does not appear to be excessive. To sufferers from chronic or recurrent affections of the respiratory organs, Parknasilla in the winter and early spring months would appear to be indicated as a most desirable place of residence. I have had the advantage of two recent visits to this district, and I feel convinced that when it becomes better known, Parknasilla will prove a veritable haven of health and rest to the chronic invalid and the convalescent, as well as a delightful retreat to the busy man of the "world's mart," who may need a temporary

repose from the worries and cares of daily life. Parknasilla is about a two-hours' drive or thereabouts from Kenmare, the drive being one of exceptional beauty and interest.

The number of health-seekers and invalids who annually leave our shores is very large, and the amount of capital that is thus lost to the home health-resorts is very considerable. Here at our very threshold we have several health-resorts—notably, Glengarriff, Parknasilla, and Rostrevor, which, from a climatic point of view, afford special advantages to the invalid world at the more critical seasons of the year, and they are justly entitled to our warmest consideration and support.

REMARKS ON RURAL WATER SUPPLIES IN IRELAND.

By D. EDGAR FLINN, D.P.H., F.R.C.S., M.R.C.P.;
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[Read in the Section of State Medicine, April 17, 1896.]

It has truly been said that, next to the food of a people, water is the most important requirement for every community; and an abundant supply of pure water is not only of primary importance from a sanitary point of view, but one of the greatest blessings of life; and, *per contra*, no more fertile source of disease exists than impure and contaminated water.

In Ireland the character of the water supply in a large percentage of small towns and villages is not what it ought to be, and this is in great measure due to the fact that the wells from which the supply is derived are nearly always situated within a congested and closely-inhabited area. In many villages wells are sunk in the very centre of a number of cottages, and often in positions that conduce to the contamination of the water. The sources of supply being of a shallow nature, and on a lower level than the surrounding habitations, the soil, in due course of time, becomes so saturated with impurities that it becomes impossible to prevent pollution. These surface wells are a constant source of danger, their method of construction being, as a general rule, of a primitive character, and very little precautionary measures being taken to prevent the soakage and infiltration of surface impurities.

In dry weather these wells provide but a scanty supply, and no thought of storage during the rainy months of the year ever enters the mind of the village cottage-holder.

The fact being that as a large number of these wells, or water holes, as they might be more appropriately termed, are shallow, the depth is, as a rule, not sufficient to produce thorough oxidation of any organic impurities that have gained access to the water, and they are more or less always a source of anxiety. In gravelly and sandy soils, however, the power of oxidation is so great that the organic matters added to the water become very rapidly oxidised, and are thus rendered comparatively harmless; yet such waters are only of a second-rate quality, and are in general use in country towns and villages.

There are several important towns in Ireland which derive their drinking-water supplies from shallow pump-wells, almost all of these wells being situated in thoroughfares surrounded by a dense population. It is quite evident that drinking-water taken from these sources cannot be altogether free of impurities, and must be always regarded with suspicion; it is, however, sometimes no easy matter to carry conviction to the minds of local Sanitary Authorities *that all public water supplies should take their source at a distance from, and not within, the boundaries of a thickly-inhabited area*, the inclination of these bodies being generally adverse to any improvements that may involve any outlay. The efforts of the Local Government Board to induce Sanitary Authorities to improve the character of the water supplies to towns and villages have hitherto not quite borne the fruits that might have been anticipated; yet in many instances recently the counsel of the central authority has been followed, and powers have been sought for and obtained for the carrying out of improved water supplies.

It is notorious that a large percentage of the wells in our

country villages are sunk simply with the view of obtaining water at any hazard, and of having it as *convenient* to hand as possible, no account apparently being ever taken of the site or the configuration and condition of the soil in the immediate locality whence the supply of water is to come. The cesspools are also similarly constructed with *a view to convenience*. Now, provided the well furnishes an abundant supply of water, and the cesspool allows the liquid refuse to soak away (and thus seldom requires cleaning out), there is very little concern evinced as to what occurs or what is likely to occur unobserved beneath the surface of the ground. In course of time the well-water is discovered to have a bad taste or emits an unpleasant odour, and probably an analysis has been ordered owing to suspicious cases of illness having occurred in the immediate locality.[a] Thus it often happens that if there be a plentiful supply of water furnished by a village pump, it may be possible for offensive matters to be so diluted that no perceptible effect is produced for a considerable time, and in this way an epidemic of enteric fever or diarrhœa may insidiously get a foothold in a country district before its true cause is found out.

Again, the majority of the dwellings in country villages inhabited by the peasant and labouring classes have hitherto been built without any regard to system, comfort, or regularity. They are usually low-roofed, with small and badly-ventilated rooms—in fact, the question of architecture never appears to have been considered. Slops and refuse of all kinds are thrown on the roadway in close proximity to the cottages, decaying animal and vegetable matter abound in profusion everywhere, the liquid manure heap (often within

[a] Vivian Poore, in his "Essays on Rural Hygiene," says that "cesspools must be written down as the most immoral of all insanitary subterfuges, and their construction should be absolutely disallowed;" and, further, he mentions that "surface wells in towns the soil of which is excrement-sodden *are little better than cesspools.*"

a few feet of the entrance door) and the pigstye complete the picture of the insanitary surroundings of the village resident. In the centre of some twenty or more of this class of cottage the village pump is situated. Can it be wondered at if pollution takes place, and that surface impurities of all kinds find their way into the water?

The selection of a site for a supply of water to a village is a subject of the highest importance, and deserves much more attention than is generally given to it. As the primary object is to obtain a supply of water as pure and as uncontaminated with surface impurities as possible, the *convenience* of the position should be a *very secondary* consideration. Lathom says that in selecting a site for a well it should be borne in mind that the underground water flows in a certain direction, towards its natural outlet, which can be generally ascertained by observing the formation of the surface of the surrounding country, a favourable site being always in the neighbourhood of a river; moreover, a well should always be sunk in such a position that the underground water should flow *from* the direction of the well towards the possible sources of pollution, and not, as often happens in this country, in the reverse direction—that is, from the sources of pollution towards the well.

Anyone who is conversant with the average Irish village will have noted that the village pump is always placed in the most convenient, if not the best, position, and always in the centre of a congested area. No protection is ever afforded against surface percolation or provision made to insure the removal of surface drainage to a safe distance; the very machinery used to raise the water is of the most primitive description, and has the unfortunate propensity of being frequently out of repair.

What very frequently occurs when a well-water supply is sought for in a village has been thus accurately described:—

A common practice is to sink two holes, one a little deeper than the other. Into the shallower of the two all the sewage of a number of houses is discharged. From the deeper hole, which has been sunk below the water-line of the porous stratum, the water for drinking and domestic purposes is pumped. Very frequently these two holes are situated within a very short distance of each other. In time the contents of the cesspool gradually soak through the surrounding soil, and find their way into the well close by. As the contents of the well are pumped out they are replenished from the surrounding mixture, and thus it is not to be wondered at if such a well does not become dry even in summer. Unfortunately the percolation of excrementitious liquids through a porous soil does not rapidly impair the taste or appearance of the water;* and thus polluted water is consumed for a long period without a suspicion of its character until an outbreak of an epidemic nature rivets public attention, and the polluted condition of the water is revealed.

It has been estimated that nearly 65 per cent. of wells in country villages are more or less contaminated with some form or other of offensive and noxious matter; and in nearly every instance, through the culpable negligence or carelessness of the inhabitants, the village pump becomes so fouled that the water supply it provides is a positive menace to the public health. This condition is more likely to be the case in low-lying districts, and where the source of the supply is derived from shallow or surface wells, or where the well is situated in the midst of a thickly-inhabited area.

* Epidemics of enteric fever without number have occurred in recent years in the United Kingdom; and the cause of these outbreaks has, in a large number of instances, been directly traced to the drinking of water from polluted wells. During the outbreak of cholera in 1893 at Ashbourne, in Derbyshire, the medical inspector of the Local Government Board, Dr. Bruce Low, reported that the epidemic there was due to the consumption of specifically polluted well-water, thus recalling the virulent cholera outbreak of 1854 in London, caused by the notorious Broad-street pump well.

The Rivers Pollution Commission found that out of 429 samples from different wells, 307 were unfit for drinking purposes. These wells were, for the most part, surface wells, at a very little depth below the level of the ground, and the water in them was frequently the drainage of adjacent cesspools and privies merely clarified by passing through a stratum of soil of greater or less thickness, and mixed with rain water that had also percolated through. This slight filtration had possibly removed any unpleasantness in smell or taste; and as these waters were bright and sparkling they were necessarily thought pure and wholesome, and were used accordingly.

A well-known American authority[a] on questions of public health, referring to the always recurring danger of water pollution in shallow wells, says:—"It is quite evident that in the case of shallow wells, if a cesspool be situated within the circle of influence of a well, any liquid which soaks from it into the ground will be likely to find its way into that well, unless the amount of liquid be too small to saturate the soil into which it soaks. In this case the water is held by capillarity, and is slowly evaporated, leaving in the soil a continually increasing amount of objectionable organic matter which is liable, on the occurrence of heavy rains, to be washed into the well. This explains what has actually been found to be the fact—namely, that some wells are noticeably bad after heavy rains, while at other times the water seems to be good. On the other hand, where there is a more direct passage of offensive matter into the well a heavy rain may dilute the water so that it will be better than in a dry time." This accords with the experience of many, no doubt, in country districts in Ireland.

In fine, many village pump-wells are suffered to remain in an unprotected and neglected condition; no systematic

[a] Professor Ripley on Drinking Water.

method of inspection of these structures is strictly ever enforced or thought advisable; and it is much to be regretted that more care is not exercised by the Sanitary Authorities as to the manner in which the contracts for the supply of water are executed, as frequently (through inexperience or incompetence on the one hand, on the part of a contractor, and from the lack of skilled supervision on the other) the supply is often scanty and defective in nearly every particular.

Happily there appears to be a disposition on the part of local public bodies to initiate the more modern methods of obtaining pure water supplies, and it becomes more apparent now that this question of sanitary reform in Ireland is gradually, if but slowly, approaching to a period of certain solution. Time, that potent solvent of all things, will, let us hope, convince our public boards that a supply of pure drinking-water is as necessary for the well-being of the individual as is the air we breathe or the food we eat.

SECTION OF ANATOMY AND PHYSIOLOGY.

TOPOGRAPHICAL ANATOMY OF THE SPLEEN, PANCREAS, DUODENUM, KIDNEYS, &c.*

By A. BIRMINGHAM, M.D.;
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During the past year I have hardened the viscera *in situ* in three subjects. So thorough was the hardening in one of these, that the firmer organs, such as the kidneys, were almost of the consistence of wood, while the pancreas and suprarenals were perfectly fixed and retained their form even under pressure. As most of the viscera were fairly normal, I thought it well, for teaching and other purposes, to make a cast of this specimen. I accordingly had one prepared, which, owing to the successful hardening, gave a most faithful reproduction of the original, even to the "texture" of the viscera in the case of the pancreas and spleen. A picture of the cast, reproduced by a photographic process, accompanies this paper; and the following pages will be devoted chiefly to a description of the organs as they appear in the cast, with some remarks on their topography as illustrated by it and by the other specimens which I have prepared. I need hardly add, that no illustration can give a true idea of such a structure as a cast of the abdominal viscera with its varying outlines, its projections and recesses.

* Illustrated by a cast of these viscera hardened *in situ*.

To make out all the points referred to here, the cast itself must be consulted.

I shall first give the method followed in preparing the specimen and making the cast; the description and remarks will follow.

METHOD.

The body, that of a middle-aged male, was placed on its back and properly supported.

I felt that the shape of the plastic viscera must be modified to a slight degree by the pressure of the overlying parts when the body rests in this position; still, I was of opinion that equally weighty objections might be made to every other position—to the erect, on account of the loss of tone in the abdominal wall; and to the prone and lateral, for the same reasons as when the body lies supine. On a consideration of the pros and cons of the case, I came to the conclusion that the latter was as satisfactory as any other position. At the same time, I believe it would be well to check the results obtained with the body in one position by comparison with others obtained under different conditions.

The left common carotid was opened in the neck, and a tube tied in the proximal end—the distal end was ligatured. The internal jugular vein of the left side was also opened, and subsequently that of the right. The openings in the veins were controlled by clips; and the object of making them was, of course, to allow of the return of injected fluids. The fluids were injected entirely by pressure from a can placed about two feet above the level of the table on which the body lay. The blood vessels were first washed out by a few quarts of normal saline solution. If this be not done, the chromic acid solution used for hardening coagulates the blood in the vessels and prevents a free circulation. After

the washing out a solution of chromic acid in water, varying in strength from ½ to 1 per cent., was injected constantly (by pressure, as explained above), for three weeks, the clips on the jugular veins being occasionally relaxed to allow excess of fluid to escape. At the end of three weeks the injection was stopped. There was by this time a distinct increase in the prominence of the abdomen and in the size of the extremities, &c., owing to the infiltration of the connective tissue spaces generally, but this in no way interfered with the shape and relations of the viscera, which appeared to be

Fig. 1.

little or not at all affected. The abdomen, and subsequently the thorax, having been opened, it was found that the viscera

were perfectly hardened, and that such structures as the diaphragm were absolutely fixed.

The abdominal cavity having been fully laid open without any disturbance of the contents, a photograph was taken. The great omentum was then removed, all other parts being undisturbed, and another photograph secured. In this way, removing part by part, and photographing at each step, the state of things shown in the illustration was arrived at. It would, of course, have been well if the stomach, liver, and colon could have been preserved, but were this done little more than these viscera would be seen; for this reason it was decided that a cast in which the viscera mentioned were removed, and only those lying on the posterior abdominal wall preserved, would be more useful. The great vessels were exposed by removing the mesentery and the peritoneum of the lower portion of the posterior abdominal wall. However, the exact peritoneal relations of the right kidney and suprarenal, of the diaphragm, pancreas, spleen, and some other parts were carefully preserved.

The abdomen having been thus prepared, there was made from it a "piece mould," bearing the exact impression of the various parts of the specimen. From this was subsequently obtained the "original" cast.[a] The original I went over in every detail, smoothing off irregularities, &c., in the usual way, and from it was taken a gelatine mould, in which the final casts, such as that shown in the illustration (Fig. 1) were made.

DESCRIPTION AND REMARKS.

Liver.—None of the liver has been retained in the cast, for reasons already explained; but in removing it the whole of the inferior cava was carefully preserved. The portion of

[a] This I handed over to Messrs. Casciani, of Wellington-quay, Dublin, for publication.

the diaphragm in direct contact with the gland is clearly indicated, as are the termination of some of the hepatic veins, and the recess in which the Spigelian lobe lay. This latter recess is, owing to the preservation of the cava, very distinct as a marked depression, bounded by the prominent vena cava on the right, and on the left by a prominence of the diaphragm, produced by the œsophagus piercing it above, and by the aorta lying behind it lower down. The illustration shows this *recessus Spigelii* indistinctly; but in the cast it is very well marked, and is seen to be lined by the posterior layer of the upper end of the small peritoneal sac. In removing the liver, I first cut the small omentum and freed the left lobe from all its connections; on lifting it forwards and to the right, I came to the recess mentioned, which was completely filled by a process of liver having a free anterior surface; I separated this piece of liver easily from the rest of the organ by cutting along its right border, that is, along the inner border of the cava; and having removed the rest of the gland, I proceeded to examine this process in detail. I recognised it as the Spigelian lobe, but I found that it differed from the Spigelian lobe of our text-books and His's models, as I understand them.

The Spigelian lobe is described and represented as an oblong mass sculptured on the posterior surface of the liver at the side of the cava, and only slightly marked off from the rest of the organ except at its lower end, where it projects prominently downwards. That is to say, it is merely a part of the surface of the liver marked off by certain bounding lines, and *not* a prominent projecting mass, almost completely cut off from the rest of the gland by the near approach towards one another of the fissure of the ductus venosus running outwards to the right, and of the vena cava projecting forwards into the liver substance. The

latter description, on the other hand, applies exactly to the Spigelian lobe in the specimen from which the cast was prepared, and also, I believe, to the average state of the lobe when *in situ*. It has a posterior surface directed backwards and a little to the left, which is lined by peritoneum and fits into the "recessus Spigelii" described above; and an anterior free surface (of greater extent than the posterior, as a rule) looking forwards and to the left, covered by peritoneum, and forming the posterior boundary of a fissure which is produced by the folding over towards each other of the Spigelian and left lobes, and at the bottom of which is found the ductus venosus. The fissure, taken altogether, cuts outwards and a little forward to within about 2 centimetres of the front of the cava; and, as will be seen on reference to Fig. 1, this lobe is separated from the rest of the liver, with the exception of an isthmus about two centimetres in width, by the fissure in front and by the cava on the right. A rather sharp left border separates the anterior from the posterior surface. This condition of the lobe, differing very much as it does from my reading of His's model, is altered when the liver is removed from the body by the opening up of the fissure and the flattening out of the Spigelian lobe, the form with which we are so well acquainted being thus produced.

Otherwise, I have found the liver to conform very closely to Professor Symington's description of the organ in Quain's "Anatomy." I have, however, been much struck by its great depth in the vertical direction on the right side, which was partly due in this case to the over-distension of the transverse colon, which was filled with gas and doubled up over the front of the empty stomach. As a result, the liver was pushed over, and packed into the right side of the abdomen, with a consequent increase in its vertical depth—

the whole showing very clearly the plastic nature of such viscera as the liver, and the way in which they may be moulded by the pressure of adjacent organs. Though this moulding may take place to a considerable extent, still it never, I believe, robs the organ of its characteristic, and what I may call its fixed, shape. There are those who hold that the forms of the organs obtained by hardening *in situ* are not really fixed forms, but that the viscera are plastic, and take for the time being, as a result of the pressure of surrounding structures, shapes which are artificially fixed by the hardening agent, and thus fictitious forms arrived at. Any one who has hardened the viscera in a number of subjects, and observed the regularity with which the definite form of each organ appears, cannot accept this view. It is, I think, much more probable that every organ has a definite fixed form; that it is plastic to a certain degree; and that it can be moulded by the pressure of adjacent organs within certain limits, producing modifications of its fundamental form.

Stomach.—In the cast only the ending of the œsophagus and a small part of the stomach close to the pylorus are shown. The rest were removed before making the cast. However, several points in connection with the relations of the organ can be made out. In the specimen from which the cast was prepared, and, unfortunately, in the other subjects which I hardened, the stomach was nearly empty. I have, however, carefully studied the condition of the stomach in these specimens, and I have arrived at the following conclusion:—

The empty or nearly empty organ is of an attenuated and modified pear-shape, not flattened from before backwards, as often figured. The wider end of the pear is directed backwards, and somewhat to the left. At the right side of this

end, a considerable distance from its summit—in fact, almost as far from the summit as from the pylorus, and towards the upper (anterior) surface—the œsophagus enters. The long axis of the organ is directed at first, and for the greater part of its extent, nearly horizontally forwards, with an inclination to the right. For a distance of about 8 or 9 cm. from the pylorus the stomach scarcely exceeds the duodenum in diameter, beyond this, it expands and passes on into the body of the organ; about the junction of this narrow pyloric portion with the rest, the axis of the pear changes its direction; it is first strongly bent towards the middle line (a little to the right of which the pylorus is placed) to reach the pylorus, and about 4 or 5 cm. nearer to the pylorus there is a second bend (not so strongly marked) downwards over the "omental tuberosity" of the pancreas, as will be explained more fully when speaking of the "stomach bed." I believe this to be the usual condition of the empty or nearly empty stomach—a condition brought about by its inherent shape and the tonic contraction of its walls. Moreover, I can scarcely conceive how the organ could ever assume a flattened form with its anterior and posterior walls in contact, particularly when the surroundings of the stomach are considered.

It should be added to the foregoing, that the lesser curvature was directed to the right and backwards, the great curvature forwards and to the left, and that at its right end this curvature lay immediately along the top of the omental tuberosity of the pancreas. This latter point will be referred to again when speaking of the pancreas.

Although I was not fortunate enough to meet with a distended stomach in any of the bodies which I hardened, I have nevertheless considered, when studying my specimens, how the organ would be influenced by distention. This, how-

ever, I had better postpone until I have referred to some points bearing on the relations of the stomach.

Nothing has attracted my attention more, in connection with the stomach, in all the specimens which I have hardened, than the striking and well-marked shelf-like arrangement which supports, at least, the undistended stomach, and which I have referred to elsewhere as the "stomach bed"—appropriately, too, as will be seen on a reference to the cast. This stomach bed is formed in the following way. The portion of the pancreas to the left of the middle line has, as will be shown later, a very considerable antero-posterior thickness; as a result, its upper surface (the anterior of His) is of considerable extent, it looks strongly upwards, and forms a large and important part of the shelf. As the pancreas passes to the left towards the spleen, it crosses the upper part of the kidney, and is so moulded on to it that the top of the kidney forms an extension inwards and backwards of the upper surface of the pancreas, and extends the bed in this direction. (This is not so well shown in the cast, owing to the low level of the left kidney.) On the other hand, the extremity of the pancreas comes in contact with the spleen in such a way that the plane of its upper surface runs with little interruption upwards and backwards into the concave gastric surface of the spleen, which completes the bed behind and to the left, and running upwards, forms a partial cap for the wide end of the pear (stomach.) To a certain extent an anterior prolongation of the bed is formed by the transverse mesocolon, which, passing forwards from the anterior margin of the pancreas, is stretched over and supported by underlying coils of small intestine in such a manner that it forms a little platform in extension of the upper surface of the pancreas in an anterior direction. Thus, with the addition of blood vessels and connective tissue, which fill the intervals between the

several parts and the covering of peritoneum, the bed is completed.

There is, moreover, a roof to this space in which the stomach lies, quite as distinct as the bed, though more simple in its constitution, and better recognised. It is formed above chiefly by the diaphragm and the under surface of the left lobe of the liver; anteriorly, the abdominal wall closes in the space; while the cap for the wide end of the pear is formed, as mentioned already, by the spleen below, and above this by the diaphragm curving upwards and forwards from behind the spleen. In this way there is formed a perfect, and I believe an almost constant, "chamber," in which the undistended stomach is contained. A striking demonstration of this can be obtained by removing the stomach very carefully, in a hardened subject, without displacing the liver, diaphragm, or other organs. Into the chamber, at times, part of the transverse colon may find its way. Of all the walls of this chamber there is none more interesting, or more definite, I believe, than the stomach bed.

As to the condition of the distended stomach. If I am correct in my contention that the undistended organ is in shape that of an attenuated and modified pear, that it lies in a chamber such as I have described, with its great curvature directed forwards and to the left, its long axis for the most part nearly horizontal and lying in a direction approaching the antero-posterior, and its wide end looking almost directly backwards, it follows, I think, that there can be no great change of direction in the axis when the organ is gradually filled. That is, I can scarcely see how the long axis could assume an almost vertical direction in the greater part of its extent, as usually described, without such a disturbance of the surrounding viscera—flattening of the pancreas against the posterior abdominal wall, &c.—as I think unlikely to

take place unless as a result of extreme distention, or a pathologically dilated stomach. On the other hand, I am inclined to the opinion that when the stomach is gradually and moderately distended, it enlarges chiefly in a forward direction, and by that extension to the right which is usually taught.* This, too, would correspond to the fulness of the "pit of the stomach" which is noticeable after a large meal. Further, I believe that there is an expansion of the organ in an upward, and to a greater extent in a downward, direction, the former accompanied by an elevation of the diaphragm and left lobe of the liver, the latter by a descent of the stomach bed. In other words, the empty stomach, on being gradually filled, does not change its "lie" to any marked extent, except in regard to the movement of its pyloric end to the right; its axis is comparatively slightly disturbed, but there is a general expansion in all directions, particularly forwards and to the right, as here the walls of the chamber in which the stomach lies are least resistant. If any great enlargement in the vertical direction take place, with increased distension there must be a considerable flexure in the back of the organ, corresponding to the position of the pancreas.

Spleen.—In the specimen from which the cast was made the spleen was somewhat enlarged. This may possibly account for the displacement downwards of the kidney on this side, and the corresponding displacement of the suprarenal. In every other regard the spleen corresponds exactly with the description of the organ given by Professor Cunningham in the *Journal of Anatomy and Physiology* a short time ago. Its relations are perhaps a little modified by its slight enlarge-

* This is largely due to the enlargement of the pyloric portion which, as explained above, is little greater in size than the duodenum when the stomach is empty.

ment, but not sufficiently to vitiate in any way the agreement with the description mentioned. Its relation to the phreno-colic ligament is well shown.

It can also be clearly seen in the cast that the inner and posterior part of the base rests on a fold of peritoneum, continuous with the left end of the transverse mesocolon, which passes inwards to the kidney from the splenic flexure, in much the same fashion as the phreno-colic ligament passes outwards. This is a relation which I have not looked for in time in my other specimens, and I cannot say how constant it may be; but it seems likely to be present with the blunt form of the tail of the pancreas, mentioned later on. It is seen indistinctly in the illustration, filling the interval which is apparent between the kidney and the colon below the pancreas, and opposite the phreno-colic ligament. In the cast it is very distinct, as the bulging of the base of the spleen covered by the fold is clearly shown in this interval.

Duodenum.—The most striking thing about the duodenum in the cast is the fact that nearly half of its curve lies in a plane not far from the sagittal, the remainder being nearly in the coronal plane. As I have found this to be the case in all my specimens, I would direct special attention to it, particularly as the duodenum is generally described and figured as if it lay practically in the coronal plane. In His's model, too, particularly when viewed from the front, the same idea is preserved.

I should explain here, that after removing the stomach, and also the jejunum immediately beyond the duodeno-jejunal flexure, I carefully stopped the lower end of the duodenum, and poured in through the upper end enough liquid gelatine to give the tube solidity, and to increase its rotundity, as it would be very difficult to get a good impression of the empty tube, which might collapse under the

pressure of the plaster. Besides, we generally think of, describe, and picture the duodenum in a more or less distended, not in an empty condition. In pouring in the gelatine, the greatest care was taken not to disturb in any way the relations of the organ to the surrounding structures—of which, indeed, owing to the thorough hardening, there was little danger.

The pylorus lies a little over 2 cm. to the right of the middle line. From this the first part of the duodenum runs transversely to the right for half its length, that is, until it has cleared the bile duct, with the hepatic artery and portal vein (which structures can be seen in the illustration running up from behind the duodenum). Beyond this it bends sharply backwards, at the side of the cava, and then turning down, passes into the second stage. The second stage descends along the right side of the cava—not anterior to it, as usually taught—lying, of course, in front of the inner margin of the kidney. It comes into very close contact with the ureter after that tube leaves the hilum of the kidney; and for fully two inches at the junction of the descending and transverse stages it lies on the psoas muscle. At the lower end of this second stage the gut bends gradually forwards and towards the middle line, forming the beginning of the transverse stage; about the middle of the transverse stage it again enters the coronal plane, in which it runs to the duodeno-jejunal flexure. The latter part corresponds very closely to the received description. The special points in reference to the duodenum brought out by the cast, to which I would direct attention, are then as follows:—(1) This loop of gut does not lie in the coronal or any one plane, as usually represented; the first half of the first stage and all beyond the middle of the transverse (or third stage) lie practically in the coronal, while the rest of the duodenum is moulded round

the right side of the cava and vertebral column, and is placed almost in the sagittal plane, the transition from one plane to the other being more abrupt above, more gradual below. (2) The cava does not lie behind the descending duodenum, but entirely to its inner or left side. This I have found not only in my specimens with the organs hardened *in situ*, but also in frozen sections. (3) The close relation of the descending duodenum posteriorly, to the ureter and the right psoas for a considerable distance. With a distended stomach and a pylorus thrust to the right, probably the first part of the first stage would lie further from the coronal plane than is the case in the cast.

Pancreas.—This organ is probably more interesting than any other in the cast, partly because the pancreas changes so rapidly after death that there is rarely an opportunity of examining one in its natural condition, and also because it differs very considerably from His's model, and from the ideas which are, I think, generally held regarding its shape. However, as I have found the pancreas practically the same in my other hardened specimens, I believe the condition shown in the cast to be normal. The head and neck agree very closely with Professor Symington's description of these parts, as found in the 10th edition of "Quain's Anatomy," with this slight difference, that the first stage of the duodenum lies above the head—not above the neck—and the pylorus lies above the neck, close to its junction with the head. It is in the *body* chiefly that the pancreas of the cast differs from the usual descriptions: first, in the distinct way in which it projects forwards as a prominent ridge into the abdominal cavity; secondly, in its great size, for it appears to be the most bulky part of the gland; thirdly, in the great extent of its under surface; and lastly, in its distinct sweep backwards round the top of the kidney as it passes to the left, as well as in several minor details.

His's model represents the body of the pancreas as being flattened against the posterior abdominal wall, with anterior and posterior surfaces of considerable extent, looking nearly directly forwards and backwards, and with a very narrow inferior surface. In all my specimens, on the other hand, the body of the pancreas, instead of being flattened from before backwards, projects very prominently into the abdominal cavity by its anterior border, so much so that its upper surface contributes very largely to the formation of the stomach bed, as already explained. This portion of the gland is very distinctly prismatic, particularly towards the tail end; but, contrary to the usual teaching, I find that the three surfaces are of nearly equal extent, the inferior being practically as large as the superior (anterior of His). The posterior surface, of course, looks backwards, and has the relations usually described. The superior surface, generally known as the anterior, looks upwards and forwards (sometimes more upwards than forwards), is of considerable extent, shelf-like in appearance, and constitutes an important part of the stomach bed. It is slightly concave, corresponding to the convexity of the stomach. The inferior surface looks downwards and forwards; it is, as explained, of about the same extent as the superior; it rests at its left end on the termination of the transverse colon, at its right on the duodeno-jejunal flexure, and between the two it rests upon, and is apparently supported by coils of jejunum. The superior is separated from the inferior surface by a sharp and prominent anterior border, which is gradually rounded off as the neck is approached. (In the cast it is well marked at the left end only.) The omental tuberosity at the right end of the body is distinct, but it does not project against the lesser omentum—at least, when the stomach is empty; on the contrary, it appeared in great part below the great

curvature—the stomach, as explained before, bending down over it.

The foregoing is the state of the body exhibited in the specimen from which the cast was taken, and it agrees in all essentials with my other specimens. I believe that it gives the true state of the pancreas when the stomach is empty; for, as already mentioned, the stomach was practically empty in all the bodies hardened. I must admit, however, that it is conceivable that a fully distended stomach might press back the organ against the posterior abdominal wall, to some extent flattening out the superior surface, and making it more anterior in direction, and reducing the inferior surface to much narrower limits; but I think, at the same time, that this is highly improbable.

The tail of the pancreas, I have found, may take one of two chief forms. It may be long and attenuated, when it runs backwards along the basal surface (Cunningham) of the spleen, curving in this course round the outer aspect ("anterior surface") of the kidney, and covered, when viewed from the front, by the splenic flexure of the colon. On the other hand, it may be stout and blunt, when it abuts against the lower part of the gastric surface of the spleen. The latter of these two forms is represented in the cast.

Kidneys.—In the specimen from which the cast was taken, the kidney of the right side lies at a higher level than that of the left, contrary to the general rule. This may be due, in part at least, to the extra pressure of the enlarged spleen, reinforced, perhaps, by a small amount of pleuritic effusion which was present on the left side of the chest, disturbing the balance of pressures which (according to Prof. Cunningham's views lately expressed in the journal quoted) is one of the chief factors in keeping the kidney in position. On

the left, also, the hilum of the kidney looks more strongly forward than on the right side, and than normal. This might, perhaps, be attributed to the depression of the kidney, which, by increasing the tension on the renal vessels, would naturally draw the hilum round to the front. An additional renal artery and its accompanying vein on this side call for no special remark.

The peritoneal relations of the right kidney are carefully preserved in the cast, although not shown in the illustration. About the upper $\frac{1}{8}$ or $\frac{1}{9}$ of the anterior surface is uncovered by peritoneum, and was in direct contact with the liver; the lower $\frac{1}{4}$ or thereabouts is also uncovered, and was connected to the back of the colon. The intervening part is covered by peritoneum, and was overlapped by the liver. In other respects the kidneys agree with Professor Cunningham's and the text-book descriptions, and call for no special notice. Attention should, however, be called to the small amount of left kidney which appears above the pancreas in the cast: this, of course, is not the normal state, and is due entirely to the low level occupied by this kidney.

Suprarenal Bodies.—In the cast the right corresponds to the description of Rolleston, with the following slight differences:—(1) The surfaces look more strongly in and out than in his description. (2) The capsule lies nearly entirely posterior to the cava, and can scarcely be seen when the cast is viewed full from the front. Hence it does not appear in the illustration. And (3) the peritoneum covers only a comparatively small portion (about $\frac{1}{5}$) of the anterior surface near the kidney. The vein is indistinctly shown in the cast, high up; it is very short, and enters the back of an hepatic vein (seen near the top of the suprarenal) which joins the right side of the cava. The left suprarenal is hardly discernible in the illustration, just below and internal to the

upper and inner end of the spleen. It appears at first sight to differ very much from Rolleston's account, but this is probably largely due to the depression of the kidney on this side, as a result of which, owing to the removal of the support which the kidney should give to its upper end, the long axis of the capsule is placed nearly horizontally.* A part only of the gland (in the cast) appears above the pancreas. This part is crescentic in shape; it lies almost in the sagittal plane, with its ends forwards and backwards; and the whole structure is moulded to the side of the left crus of the diaphragm, the side in contact with the crus being slightly concave, the other correspondingly convex.

I believe that the surfaces of the suprarenal capsules look much more in and out than backwards and forwards, as usually described. In fact, they lie very close to the sagittal plane.

Large Intestine.—Only the splenic flexure, the descending colon, a small part of the sigmoid flexure, and the rectum have been preserved; the other parts were removed, in order to show the structures which lay behind them. Attention has been already called to the connections of the splenic flexure when speaking of the spleen. It need only be added that the flexure does not ascend as far as usual into the hypochondrium, owing to the enlargement of the spleen. Nothing in the descending colon calls for remark; it agrees exactly with the description in the latest edition of Quain's Anatomy. The cast shows clearly the line of attachment of the sigmoid mesocolon. The upper part of the rectum is fairly distinct, but the remaining contents of the pelvis are only slightly indicated.

Œsophagus, Ureters, &c.—The lower end of the œsophagus

* This is contrary to Rolleston's view. He maintains that the position of the suprarenal is not affected by changes in the position of the kidney.

has been preserved; its opening in the diaphragm is well shown, as are the left and part of the right crura of the diaphragm.

Both ureters are shown clearly as far as the brim of the pelvis.

Below the level of the kidneys and the duodenum, the peritoneum has been removed from the posterior abdominal wall without preserving the lines of reflexion, except at the sigmoid mesocolon, and in the pelvis, where the peritoneum remains undisturbed. Above, its relations to the kidney, suprarenal, and diaphragm on the right side are carefully shown. The posterior layer of the upper part of the small sac, opposite the Spigelian lobe, is very distinct, and the right phrenic vessels will be observed passing upwards and outwards beneath it on the diaphragm. As already explained, the phreno-colic ligament passing out from the splenic flexure, and a fold (connected with the last bit of the transverse mesocolon) passing inwards from it, are preserved; the latter forming, in this case at least, a sustentaculum lienis internum.

The attachment of the transverse mesocolon is indicated by a coloured line. The two layers are separated by about 1½ inches on the descending duodenum; they gradually approach as they pass to the left on the head of the pancreas, and they finally meet at the neck; beyond this the line pursues a course which corresponds fairly closely to the anterior margin of the pancreas until the left end of the body is reached, when it passes on to the under surface. Consequently, in the specimen from which the cast was taken, the back of the transverse colon was connected to the front of the descending duodenum at about its middle, and to the adjacent part of the head of the pancreas by connective tissue without the interposition of peritoneum, corresponding in this very closely to the condition shown in His's model.

Vessels, &c.—The true proportion between the size of the arteries and the veins is shown—a matter of some interest, as this is the reverse of what is seen in ordinary dissecting-room subjects, in which the arteries are injected, and the veins are empty and insignificant. The aorta calls for no remark. The cava has been preserved in its whole length by removing the liver without disturbing the vessel; thus what is perhaps an unusual view has been secured. Several hepatic veins are shown joining it, and there is still noticeable a part of a very strong flexure to the left which engaged the cava just as it approached the auricle; part of this flexure has been removed. The hepatic artery, hepatic duct, and portal vein are well shown in their characteristic position just above the first part of the duodenum. The hepatic artery can be followed to the cœliac axis, from which can be seen arising the stump of the gastric, and also the splenic. The extremely tortuous course of the latter vessels along the upper border of the pancreas is well seen, and a part of its companion vein can be made out in the cast (not in the illustration) lying inferior and posterior to the artery, on looking down behind the upper margin of the pancreas.

The superior mesenteric vein has been preserved down as far as the middle of the transverse duodenum; the corresponding artery is cut short to show the head of the pancreas winding to the left beneath the vessels. This recurved piece of the head runs as far as the left margin of the superior mesenteric artery. The gastro-duodenal artery is seen coming down behind the duodenum, quite close to (indeed, practically behind) the pylorus, and dividing into the right gastro-epiploic, which is cut short and pinned down to the pancreas (to prevent its being injured in making the mould), and the superior pancreatico-duodenalis, which runs downwards in a groove on the front of the head, a considerable distance

from the duodenum, as described and pictured by Symington in "Quain's Anatomy." The corresponding vein is seen running downwards and to the left, to join the superior mesenteric. The so-called inferior pancreatico-duodenal artery is not shown, for it lies on the *back* of the head, running downwards and to the right, behind the recurved part of the head. The names "anterior" and "posterior" pancreatico-duodenal would be less misleading than those at present used. The remaining vessels call for no special remark.

A small ridge is seen running out from the side of the right psoas muscle; it is not indicated in the illustration, but in the cast it is very distinct, and is due to a raising up of the iliac fascia by a small branch of the ilio-lumbar artery, often seen in this situation. It is not to be mistaken for the crest of the ilium and the ilio-lumbar ligament, which lie at a higher level, about 1½ inches above this ridge.

CONCLUSIONS.

The chief points which I have called attention to in the foregoing may be briefly summarised.

1. The lobus Spigelii of the liver has an anterior free surface as well as a posterior, and it lies in a special "recessus Spigelii" on the posterior abdominal wall. The opinion is expressed that such viscera as the liver have definite fixed shapes, which may be modified but not destroyed by the ordinary pressure of adjacent organs.

2. The empty stomach is in shape that of an attenuated and modified pear, with its wide end directed backwards and to the left; the greater part of the long axis runs forwards and a little inwards in the horizontal plane, and turns to the right and downwards a few inches from the pylorus. Its great curvature looks forward and to the left, its lesser to the

right and backwards. Special attention is called to the "stomach bed," which forms the floor of a chamber in which the stomach lies.

3. The spleen corresponds to Professor Cunningham's description. In the cast a *sustentaculum lienis internum* is present, formed in connection with the left end of the transverse mesocolon.

4. The duodenum does not lie in the coronal plane, as usually represented; the first half of the first stage and all beyond the middle of the transverse stage lie approximately in the coronal plane, the intermediate part is moulded backwards by the side of the cava, and lies almost in the sagittal plane. The cava does not lie behind the descending duodenum, but to its left. The duodenum is also closely connected behind to the right ureter and psoas.

5. The pancreas agrees as to its head and neck with Symington's description. The body of the pancreas projects forward as a distinct ridge into the abdominal cavity, and is not flattened against the posterior wall. It is prismatic, but the three sides of the prism are nearly equal, the inferior surface being as extensive as the superior(or anterior). Two forms of tail are described—one which is long and slender, and curves back beneath the basal surface of the spleen; the other short and stout, which abuts against the lower part of the gastric surface of the spleen. The omental tuberosity, with an empty stomach, does not come in contact with the lesser omentum, but lies in great part below the great curvature of the stomach.

6. The suprarenal bodies lie not very far from the sagittal plane; and the right is nearly entirely posterior to the vena cava.

7. The position of the inferior pancreatico-duodenal artery on the back of the recurved part of the head of the pancreas

is described, and the names "anterior" and "posterior" are suggested instead of superior and inferior pancreatico-duodenal, as probably being more correct.

Several minor points are touched on, which need not be mentioned here.

The President said they had reason to feel obliged to Prof. Birmingham for the great trouble he had taken in the preparation of his cast. He had spent, he said, some time in considering the positions and relations of the abdominal organs, and to acquire accurate knowledge in this respect was a matter of great difficulty. On these questions there could be no doubt but that the opinions of the older anatomists are extremely fallacious. He felt bound to protest against many statements in reference to the relations of the abdominal viscera, contained in text books. It was the duty of anatomists, so far as lay in their power, to supply physicians with accurate descriptions of those viscera. He agreed with the author that when empty, a greater part of the stomach was horizontal in position. As regards closure of the pylorus, it was not effected by contraction of the pylorus alone, but by contraction taking place about an inch on the cardiac side. He believed there was a good deal of work to be done in regard to the pathology of the abdominal organs, for instance, in tracing the effect of enlargement of one organ upon neighbouring organs. He pointed out that an enlarged liver may not push down the right kidney, the hepatic enlargement passing down in front of the latter organ. He believed the frozen section method employed by many anatomists was a source of mistakes. He expressed a high opinion of formalin as a hardening agent.

ON THE HOMOLOGY OF THE DUMB-BELL-SHAPED BONE IN THE ORNITHORHYNCHUS.

By JOHNSON SYMINGTON, M.D., F.R.S.E.;
Professor of Anatomy, Queen's College, Belfast.

[Read in the Section of Anatomy and Physiology, January 24, 1896.]

ALTHOUGH this curious little bone, situated in the snout of the duck-mole, has been examined and described by various anatomists, its homology is still disputed. The theories that have been advanced on this subject are, (1) that it represents the inner parts of the two intermaxillary bones; (2) that it corresponds to the prenasal bone in the pig; and (lastly) that it is an anterior vomer. The first-mentioned view has been supported by Rudolphi, Meckel, Owen, Cleland, Albrecht, Turner, and myself; the second by Owen and Flower; and the last by J. T. Wilson and R. Broom.

As Sir W. Turner in a paper[a] in the Journal of Anatomy and Physiology has given the earlier literature on the homology of the dumb-bell bone, and, in my opinion, has effectually disposed of the prenasal theory, it is unnecessary for me to refer to these aspects of the question. It may, however, be noted, that Sir Richard Owen, in his article "Monotremata" in the "Cyclopædia of Anatomy and Physiology," originally supported the intermaxillary theory, but subsequently abandoned it, and in his "Comparative Anatomy of the Vertebrates" adopted the prenasal view; while Sir William Flower, after the publication of Turner's paper, gave up the prenasal in favour of the intermaxillary theory (*see* Flower and Gadow, "Osteology of the Mammalia, 3rd edition, 1885, p. 214).

[a] The dumb-bell-shaped bone in the palate of Ornithorhynchus compared with the prenasal bone in the pig. Jour. Anat. and Phys., Vol. XXI.

The dumb-bell-shaped bone is of small size, its total length being on an average about 8 mm., the greatest transverse diameter of the anterior nodule 4 mm., and of the posterior 6 mm., while the isthmus is only 1 mm. across. Further, it is not firmly fixed to any of the other skull bones. The determination of its precise relations by the ordinary methods of dissection is, therefore, attended with considerable difficulty, and can only be thoroughly investigated when these methods are supplemented by serial section and microscopic examination.

In 1891 I published a paper[a] in which the description of the bone was mainly based upon a study of serial sections; and Professor J. T. Wilson and Dr. C. J. Martin in 1893,[b] and Professor Wilson in 1894.[c] extended our knowledge of the form and relations of this bone by a very thorough and accurate description of the cartilaginous and osseous framework of the snout of the Ornithorhynchus. All these papers were accompanied by illustrations, so that we now possess the necessary data, so far as the anatomy of the dumb-bell bone is concerned, to enable us to compare it with the osseous elements of the anterior part of the face of the other mammalia.

The dumb-bell bone consists of two lobes, anterior and posterior, united, when seen from below, by a narrow isthmus, and Wilson describes a small forked process, projecting backwards from the posterior lobe, which he terms the vomerine spur. The isthmus is bounded laterally by the naso-palatine foramina. The ventral surface of the anterior lobe, the isthmus and the anterior part of the posterior lobe, are covered

[a] On the Nose, the Organ of Jacobson, and the dumb-bell-shaped Bone in the Ornithorhynchus. Proc. Zool. Soc. of London, 1891. No. XXXIX.

[b] Observations upon the Anatomy of the Muzzle of the Ornithorhynchus.—Macleay Memorial Volume. Sydney, N.S.W.

[c] Observations upon the Anatomy and relations of the dumb-bell-shaped Bone in Ornithorhynchus, with a new theory of its homology, &c.—Proc. Linnean Soc. of New South Wales, 1894.

simply by the oral mucous membrane and submucous tissue, but behind these parts the bone becomes separated from the oral mucous membrane by a layer of cartilage. On this point I wrote as follows :—

"In the greater part of its extent the bone is covered on its oral aspect by mucous membrane and a thin layer of submucous tissue, but near its posterior extremity also by a layer of cartilage. This is effected by an extension inwards to the middle line of the plates of cartilage which at the naso-palatine foramina lie external to these openings" (*op. cit.*, p. 581).

Professor Wilson has specially emphasised the morphological importance of this relation of the bone, and has also shown that it is prolonged on the dorsal aspect of the palatine plates of the maxillæ, and that its vomerine spur extends backwards in relation to the ventral edge of the cartilaginous nasal septum above the internasal passage, and within about 2 mm. of the vomer. The extremities of the bifid vomerine spur are connected with the vomer by bilateral vomerine ligaments (Wilson).

FIG. 1.

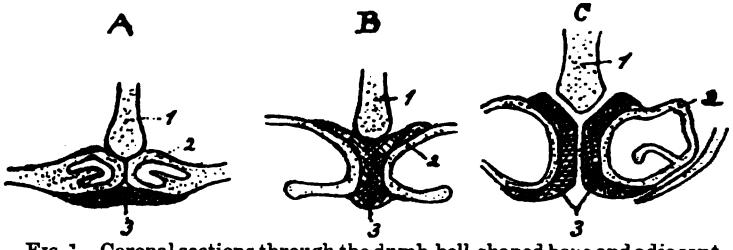

FIG. 1.—Coronal sections through the dumb-bell-shaped bone and adjacent cartilage × 8.

A., through middle of anterior lobe; B., through isthmus; C., through middle of posterior lobe.

1, septal nasal cartilage; 2, cartilage investing Jacobson's organ; 3, dumb-bell-shaped bone.

The anterior end of the dumb-bell bone is flattened from above downwards, but in the greater part of its extent the bone possesses a well-marked median dorsal ridge. This ridge begins on the anterior lobe, and opposite the naso-palatine

foramina is already well marked. Its dorsal edge divides into two lateral processes, so that on coronal section the bone appears as two crescents with their convexities directed inwards, while the external concave surface on each side lodges the inner part of the cartilage of Jacobson's organ. The lower or ventral horns of the crescent are very slightly marked opposite the naso-palatine foramina (see B., fig. 1), but about the middle of the posterior lobes both upper and lower horns are well developed, and reach fully half way round the cartilage of Jacobson's organ. The dorsal aspect of the bone possesses a median depression, which serves for the lodgment of the ventral edge of the cartilaginous nasal septum. The fusion of the lateral halves of the dumb-bell bone is by no means complete. In my specimen, an adult female, they are united as far back as the naso-palatine foramina, in such a way that no trace of a median suture can be detected. Opposite the posterior part of the isthmus a median fissure appears in the bone. This fissure begins on the ventral aspect, but is soon complete, so that the posterior lobes consist of two distinct lateral halves united merely by fibrous tissue.

The question of the homology of this bone must now be considered. As already stated, only two views need be noticed here, viz., the *premaxillary* and the *vomerine*. According to the former, "in its position and relations it corresponds with that part of the intermaxilla which lies between the incisive canal and the mesial palatine suture" (Turner). This is the part of the premaxilla termed the mesial palatine process. or the process of Stenson. Professor Wilson has brought forward various objections to this view, and contends that the dumb-bell-shaped bone "is a true 'anterior vomer,' formed of course, by the fusion of two bilaterally symmetrical halves" (*op. cit.*, p. 138). He further holds that both in its palatine and in its nasal relations it corresponds to the palatine lobes

of the vomer described by Professor Howes[a] as existing in *Caiman niger*. The situation of the dumb-bell-shaped bone in the palate internal to the naso-palatine foramina and the close relation of this bone to the organ of Jacobson are important points in favour of its homology with the mesial palatine processes of the premaxillæ of the mammalia, but the grounds on which Wilson objects to the premaxillary theory are deserving of careful consideration. In order to appreciate their force, it is convenient to regard the dumb-bell bone as consisting of a ventral portion more or less flattened dorso-ventrally, which may be termed the palatine plate, and a dorsal part which forms a vertical lamella, and enters into the formation of the nasal septum, although it must be distinctly understood that these two parts are directly continuous with one another, and show no indications of a line of separation.

The palatine plate of the dumb-bell bone does not, according to Wilson, lie in the same morphological plane with the maxillary palate, because its hinder end is covered ventrally by the cartilage of the nasal floor. This does not appear to me to be a cogent reason. The amount of bone covered in this way is slight, and amounts to little more than an overlapping of the bone by cartilage. Further, Wilson and Martin have shown that the two large bones admitted to be premaxillary elements, which are situated in the lateral parts of the beak, and articulate posteriorly with the maxillæ and nasals, are traversed by the plate of cartilage forming the anterior and lateral portions of the beak of the Ornithorhynchus. In some situations this cartilage is merely imbedded in grooves on the outer and inner bodrers of these two premaxillary bones, while in other situations the cartilage

[a] On the probable existence of a Jacobson's Organ among the Crocodilia, with observations upon the skeleton of that animal in the Mammalia.—Proc. Zool. Soc. of London, Feb. 1891.

goes completely through the bone, and divides it into a dorsal and a ventral portion (see *Macleay Memorial Volume*, pl. xxiii. fig. 16). They suggest, I think very justly, that these premaxillæ are developed on both the ventral and dorsal aspect of this cartilaginous plate, which latter subsequently undergoes partial absorption. They do not, however, imply that each of these premaxillary bones represents two distinct morphological elements. It is doubtful if any morphological significance can be attached to this relation of bone and cartilage, and this significance is still more doubtful in connection with the existence of a layer of cartilage on the ventral aspect of the posterior end of the dumb-bell bone.

Wilson considers that the prolongation backwards of the vertical part of the bone on the dorsal side of the maxillary palate, and in close relation with the nasal septum, is opposed to the premaxillary, and favours the vomerine view.

Professor John Cleland, in his classical paper "On the relations of the Vomer, Ethmoid, and Intermaxillary Bones" (*Philosophical Trans. of London*, 1861), describes the position and relations of the mesial palatine process of numerous mammals. He referred to various cases in which this process extends, as in the dumb-bell bone, backwards on the dorsal aspect of the maxillary palatine plates. Thus, in connection with the skull of the kangaroo, Cleland writes as follows:—

"The vomer exhibits on each side a peculiar lateral ridge, which extends forwards from the point where the vomerine lamina comes off and articulates in front with a very long prolongation backwards of the mesial process of the intermaxillary which extends between it and the maxillary" (*op. cit.*, p. 302).

Perhaps the most striking illustration of the backward prolongation of the mesial palatine process of the premaxilla on the dorsal aspect of the maxillary palatine process is to be found in the Tasmanian wolf (*Thylacinus cynocephalus*). In an adult skull of this animal, kindly placed at my disposal by

my colleague Professor R. O. Cunningham, the total length of the mesial palatine process is 55 mm., and the portion on the dorsal aspect of the maxillary palatine process is 42 mm. long. In Flower's Osteology of the Mammalia, 3rd ed., p. 236, there is an illustration of a median section of a skull of this marsupial which shows very well this relation of the median palatine process, but is misleading in so far as its relation to the vomer is concerned, since it represents the posterior end of the process overlapping the dorsal edge of the anterior end of the vomer. This is not the case. The two mesial palatine processes form a median dorsal furrow which anteriorly lodges the septal nasal cartilage, and near its posterior end the anterior portion of the ventral edge of the vomer.

Figs. 2 and 3 are taken from a series of coronal sections through the snout of a pouch specimen of *Macropus major*.

Fig. 2.

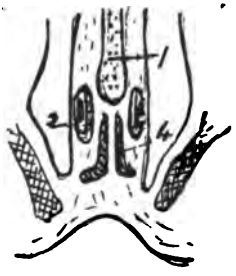

Fig. 3.

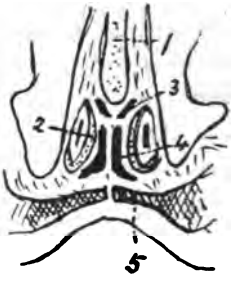

Fig. 2.—Coronal section of Snout of *Macropus major*, immediately behind naso-palatine duct. 1, septal nasal duct; 2, cartilage of Jacobson's organ; 4, mesial palatine process of premaxilla.

Fig. 3.—Coronal section of Snout of *Macropus major*, posterior to fig 2, 3, vomer; 5, palatine process of maxilla; other letters as in fig. 2.

They illustrate the relations of the mesial palatine processes to the palate, nasal septum, organ of Jacobson, and vomer. Fig. 2 is just posterior to the naso-palatine duct, and shows the dorsal extension of the process, internal to the organ of Jacobson, towards the nasal septum. In fig. 3, which is taken from a section further back, the maxillary palatine

processes are seen on the ventral side of the mesial palatine processes, while the lateral lamellæ of the vomer appear between the latter and the septal cartilage. It may be noted that the vomer is not really composed of two separate halves, as sections still farther back show it to be a single bone embracing the ventral edge of the nasal septum.

The backward extension of the mesial palatine process on the dorsal aspect of the maxillary palatine plate is not found in all the Marsupialia; thus, in a skull of Phascolarctos in my possession it terminates almost directly opposite the anterior edge of this palatine plate. On the other hand, in a number of Eutherian mammals the posterior part of the mesial palatine process will be found covered neutrally by the maxillary process. In none of them, however, do they appear to do so to the extent found in the Thylacinus.

An interesting modification in this tendency of the posterior part of the mesial palatine process to disappear from the oral aspect of the hard palate is found in the Armadillos, as in these animals the lateral palatine plates of the premaxillaries extend inwards so as to meet in the median plane on the ventral side of the mesial processes. This is shown in fig. 4 representing a coronal section through the snout of a peba armadillo.

Fig. 4.

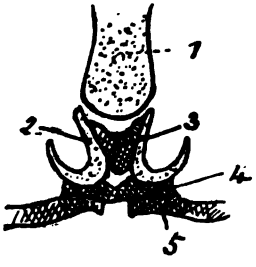

Fig. 4.—Coronal section of Snout of Peba Armadillo opposite Jacobson's organ. 1, septal nasal cartilage; 2, cartilage of Jacobson's organ; 3, vomer; 4, mesial palatine process of premaxilla; 5, lateral palatine process of premaxilla.

From these facts it is evident that the objection urged against the premaxillary theory by Wilson, on the ground that the posterior part of the dumb-bell-shaped bone is situated on the dorsal side of the maxillary palatine plates, is not valid.

As a rule, in mammals, the vomer extends forwards so as to articulate with the premaxillaries, the mesial process of the latter forming a median groove for the lodgment of the vomer. In the Ornithorhynchus this is not the case, as the dumb-bell bone is entirely anterior to the vomer, and is not, as Turner supposed, "inferior to the vomer, and fused with its lower border." This arrangement, however, is by no means peculiar to the Ornithorhynchus.

The two premaxillary bones, in mammals generally, exhibit very little tendency to become united by the ossification of the intervening connective tissue, whereas there is complete osseous union of the anterior lobes of the dumb-bell-shaped bone. In the 3-toed sloth, however, the tendency to fusion of the two lateral elements is more marked than in the duck-mole; for not only is there a single osseous nodule in the adult to represent the two premaxillary bones, but I have recently found from an examination of a fœtal specimen that it is ossified from a single centre.

If the premaxillary homology of the dumb-bell bone be true, the Ornithorhynchus is unique amongst mammals in having its premaxillary bone permanently divided into two distinct portions—a mesial and a lateral. It is worthy of note, however, that in the 3-toed sloth only the mesial portion of the bone is developed, while in certain bats only the lateral part is formed. I consider that the evidence in favour of the homology of the dumb-bell bone of the Ornithorhynchus with the mesial palatine processes of the premaxillæ of ordinary mammals is conclusive; but the question now arises, ought the mesial processes to be regarded as portions of the

premaxillary bones, or should they be referred to the vomerine series, and called "anterior vomers?" The evidence that has been advanced in support of their being "anterior paired vomers" is based upon the belief that they are formed from centres of ossification distinct from those forming the main portions of the premaxillæ, and also upon data derived from comparative anatomy.

The late Professor W. K. Parker appears to have been one of the first morphologists to describe the existence of osseous elements in the snout of various mammals, which he regarded as anterior paired vomers. Frequent references to anterior vomers and palatine processes of the premaxillæ will be found in his monographs "On the structure and development of the Skull in the Mammalia" (pt. ii., Edendata, and pt. iii., Insectivora, *Phil. Trans.*, 1886); but as Professor Howes has shown, "he was unable to draw a sharp distinction between the palatine processes of the premaxillæ; and his anterior paired (or lateral) vomers." As a rule, when the process is short, he appears to regard it simply as a process of the premaxillæ; when long, it is called an anterior vomer; while in some cases, as for instance in the mole, he regards the process as mainly premaxillary, for the antero-lateral vomers "have a very temporary and doubtful existence independent of these processes of the premaxillaries." (*op. cit.*, p. 179).

The ossification of the whole or of the posterior part of the mesial palatine process from an independent centre has not yet been satisfactorily demonstrated except in a few instances. I will not attempt to discuss here the extensive literature bearing on the number of centres from which the premaxillary bone is formed, but may state that Dr. F. Schwink* made complete serial sections of the snout of embryos of various mammals, and in only one case, that of a sheep 48 mm. long,

* Ueber den Zwischenkiefer und seine Nachbarorgane.—München, 1888.

did he find the centre for the palatine process separate from the body of the bone. Schwink very properly points out that it is easy to make sections showing the palatine process separate from the body, and that a complete series must be examined to determine whether or not it is really an independent centre.

I think it must be admitted that the mesial palatine process is generally formed, at least in part, as a process from the body of the premaxilla; and even although its posterior part may sometimes be developed from a separate centre, this is a very slender basis on which to assume that it represents an independent morphological element of the skull. The separate centre, if it exists, shows a remarkable tendency to fuse very quickly with the body of the premaxillary bone. In the entire mammalian series there is no animal, so far as I am aware, in which the mesial palatine process fails to unite with the body of the premaxilla should the latter be present. Again, there is no evidence that it fuses with the vomer so as to prolong that bone downwards and forwards to the hard palate in the region of the naso-palatine foramen. The vomer does, as Cleland has shown, occasionally appear in the hard palate, but these cases are obviously due to imperfect development of the palatine plates of the maxillæ or palates. The most important evidence from the sub-mammalian vertebrata that has yet been advanced in favour of the vomerine nature of the median palatine process is contained in a suggestive paper[a] by Professor G. B. Howes. He shows that in an Alligator of Inter-Tropical America, *Caiman niger*, the vomer appears in the hard palate, so as to be visible from below between the premaxillo-maxillary sutures, where they are "inflated and bullate." In the

[a] On the probable existence of a Jácobson's Organ amongst the Crocodilia, with observations upon the skeleton of that organ in the Mammalia. – Proc. Zool. Soc. of London, Feb., 1891.

other Crocodilia the vomers do not reach so far forwards as in the black caiman, and Howes found evidence that in them the vomer had shortened up. It was relatively long in young specimens, and in older animals a more or less powerful vomerine ligament passed from the anterior end of the vomer towards the place of meeting of the premaxillo-maxillary sutures where it blended with the periosteum. In a young *Alligator mississippiensis* he found two small foramina occupying very nearly the position of the palatine lobes of the *Caiman niger*. Within the embrace of these foramina he discovered a couple of small sacs with fibro-cartilaginous walls, and containing a "soft (? vascular) lining." There were no communications between these sacs and either the nose or mouth, and Howes concluded from their osition that they were vestiges of Jacobson's organs. As these sacs occupied a position closely corresponding to the palatine lobes of *Caiman niger*, and as the mammalian palatine processes of the premaxillæ partly ensheath Jacobson's organs, Howes considers that we are justified in regarding, at least provisionally, these two processes as one and the same element.

A critical examination of the observations and suggestions made by Howes will show that the proofs he advances in support of his theory as to the homology of these two elements are by no means conclusive. The degree to which the vomer extends forwards, and the mesial palatine process backwards, varies considerably in different mammals, and Howes shows that this is also the case in the Crocodilia. Thus in *Caiman niger* the mesial palatine process is absent, while the anterior end of the vomer is large, and forms what Howes terms its palatine lobe. In *Crocodilus palustris* the vomer does not appear on the oral aspect of the palate, but the mesial palatine process forms a spur of bone which is relatively larger in old than young specimens, and overlaps dorsally the maxilla.

Howes regards, I think correctly, the vomerine ligament in *Crocodilus palustris* as a proof of the shortening up of the vomer, and he considers the mesial palatine process of its premaxilla as a mere secondary outgrowth, having little if anything to do with the palatine process as ordinarily understood. There seems to be no proof in favour of the view that the palatine lobe becomes detached from the vomer and joins the premaxilla, and it appears much more natural to assume that the appearance of the vomer in the palate of *Caiman niger* is to be correlated with the imperfect development of the palatine processes of the premaxillæ. Various examples are found in the skull of an element appearing in places where it is usually absent, this appearance being connected with its increased growth and the associated atrophy of the neighbouring elements.

The existence of a vestige of Jacobson's organ in connection with the palatine lobe of the vomer in *Caiman niger*, which constitutes one of the main grounds on which Howes homologises this lobe with the mesial palatine process of the premaxilla of mammals, seems to me to be somewhat problematic. Howes had no opportunity of ascertaining whether or not there was a rudiment of a Jacobson's organ in *Caiman niger*, as he did not possess a spirit specimen, while the sac he found in *Alligator mississippiensis* was exposed by ordinary dissection, and its relations and structure were not determined by microscopic section of this organ *in situ*. The view that this sac was a rudiment of Jacobson's organ must be accepted with reserve, since nearly all those who have searched for a Jacobson's organ in embryo crocodiles have failed to find any trace of it. It must be admitted, however, that Dr. C. P. Slinter[a] has demonstrated its existence in the embryo of *Crocodilus porosus*. Even were it conclusively demonstrated that a rudimentary organ of Jacobson is situated in close

[a] Das Jacobson'sche Organ von Crocodilus porosus.—Anat. Anzeiger, 1892.

relation with the palatine lobe of the vomer of *Caiman niger*, this would not be conclusive proof of its homology with the mesial palatine process of the mammalian premaxilla; the organ of Jacobson does not appear to bear a definite and constant relation to any one osseous element of the skull.

The fibrous tissue found between the vomer and the dumb-bell-shaped bone in the Ornithorhynchus, Wilson describes under the name of vomerine ligaments, and regards as indicating the vomerine nature of the dumb-bell-shaped bone. He compares them with the vomerine ligaments described by Howes in various crocodiles. and appears to regard them as the fibrous representatives of the bone uniting two vomers, a posterior (vomer) and an anterior (dumb-bell-shaped bone), According to Howes, the vomer of the Ichthyopsida and lower Amniota, whether single or paired, is invariably a non-repetitional bone lying immediately behind the premaxilla. In the duck-mole the vomer is separated by a short interval (2 mm.) from the dumb-bell bone, and it appears to me more reasonable to regard the vomerine ligaments as an elongation of the sutural ligaments normally uniting the vomer and the premaxilla, rather than as a vestige connecting a vomer which had become divided into anterior and posterior portions.

Dr. R. Broom, in a paper "On the Organ of Jacobson in the Monotremata," which appeared in *Journal of Anat. and Phys.*, Oct., 1895, assumes that the palatine processes of the premaxillæ in mammals and the dumb-bell-shaped bone are "anterior vomers," and the homologues of the "vomer" in lizards. From *Nature*, 15th Oct., 1895, I find that he read a paper before the Linnean Society of New South Wales on the 28th Aug., 1895, "On the homology of the palatine process of the mammalian premaxillary," but the journal containing this communication has not, so far as I am aware, yet arrived in this country.

ASYMMETRY OF THE STERNUM.

By A. BIRMINGHAM, M.D.,
Professor of Anatomy Catholic University, Dublin.

[Read in the Section of Anatomy and Physiology, May 22, 1896].

My attention has been directed to the asymmetry of the sternum by a bone in my possession which exhibits this condition in a marked degree, and in which the want of symmetry is of such a character that it suggests as its cause the excessive use of one upper limb. On examining a considerable number of sterna I found that, as in the case of other impaired bones, the great majority were more or less asymmetrical.

It appears to me that this want of symmetry in azygos bones may generally be attributed to unequal strain on the two sides, long continued; also in certain cases to errors of development or of posture.

If the first of these be the chief cause of the irregularity of azygos bones, the sternum is placed in a position in which it is specially liable to asymmetrical changes, owing to its relation to the upper limb—forming as it does the only bony medium through which the skeleton of that limb is connected to the trunk, and also owing to the habitual preference shown for the use of one upper limb as compared with the other.

If it be admitted that the sternum is placed in a position which renders it liable to asymmetrical changes as a result of the excessive use of one upper limb, it might naturally be inferred that the prevailing habit of right-handedness would have a constant and fixed effect on it, and that as

the majority of people are right-handed, a corresponding majority of sterna would present what we may call for the moment right-handed asymmetry. This, however, as will be shown further on, is not the case.

Upon consideration it became evident that the sternum should be little, if at all, affected by right-handedness proper, for right-handedness simply means, in the majority of cases, that one hand is used in preference to the other for fine and skilled movements, but not necessarily for powerful efforts where strain is thrown on the limb. These fine movements, which only the right hand is, as a rule, capable of performing freely and perfectly, very often take place at the elbow or in the hand, and as such could produce no appreciable effect on the sternum.

When, however, any considerable strain is thrown on the shoulder, either through pressure directly applied, or through the pull of a weight carried in the hand, the outer end of the clavicle is depressed, the sterno-clavicular ligaments are rendered tense, and the inner end of the clavicle is brought to press forcibly on the facet of the sternum and the first costal cartilage. Such a condition of things occurring frequently on the same side, for a considerable period of time, would naturally produce one-sided effects on the sternum. The effects which might be expected in such cases are:—(1) A depression of the clavicular facet on the side of the excessive strain, accompanied by a diminution of the vertical depth of the manubrium on that side; (2) A diminution of the interval between the first and second rib cartilages on the same side, owing to the pressure of the clavicle upon the first costal cartilage; And possibly (3) an obliquity of the line of the symphysis between the manubrium and gladiolus.

That these conditions are frequently associated will be seen by the following Table, in which the results of an

examination of 54 sterna are set out; but, as already mentioned, the relative frequence of their occurrence on the two sides in no way agrees with the proportion of right-handed to left-handed people.

While the number of left-handed to be found among 100 average individuals is very small, the percentage of sterna showing left-handed asymmetry is very considerable—viz., 37 per cent. This, however, is easily explained by the fact that many who are naturally right-handed carry weights habitually and by preference with the left upper limb.

In examining the sterna I was struck by the frequent presence of a longitudinal curve in the whole sternum, with its convexity to one side, most marked, as a rule, towards the lower end of the gladiolus. The presence of this curve, and the direction of its convexity, is noted in the following Table:[a]—

[a] I am much indebted to Professors D. J. Cunningham, Bertram Windle, Arthur Thomson, Charles Stewart, and Dr. F. G. Parsons, for kindly allowing me to examine the specimens of sterna in their collections.

No. of Sternum	Clavicular facet depressed on	Vertical depth of manubrium diminished on	Interval between 1st and 2nd rib cartilages less on	Line of symphysis oblique, and lower on	Longitudinal curve present and convex to
1	Right	Right	Right	Not observed	None present
2	,,	,,	No difference	,,	Not observed
3	Left (slightly)	Left (slightly)	,,	,,	,,
4	Right	Right	Right	,,	None present
5	,,	,,	,,	,,	Right
6	,,	,,	,,	,,	None present
7	Symmetrical	Symmetrical	No difference	,,	,,
8	Left	Left	,,	,,	Right
9	Right	Right	,,	,,	,,
10	,,	,,	Right	,,	,,
11	,,	,,	,,	,,	,,
12	Left	Left	No difference	,,	Not observed
13	,,	,,	Left	,,	None present
14	Right	Right	Right	,,	Right
15	,,	,,	No difference	,,	Not observed
16	,,	,,	Right	,,	Right
17	Left	Left	Left	,,	Left
18	,,	,,	No difference	,,	,,
19	Symmetrical	Symmetrical	Right (very little)	,,	,,
20	Left	Left	Left	,,	Right
21	Right	Right	Right	Not oblique	Left
22	Left	Left	Left	Right	None present
23	,,	,,	,,	Left	Left
24	Right	Right	Right	Not oblique	None present
25	,,	,,	,,	Right	Left
26	,,	,,	,,	,,	Right
27	Left (slightly)	Left (slightly)	Left	,,	,,

No. of Sternum	Clavicular facet depressed on	Vertical depth of manubrium diminished on	Interval between 1st and 2nd rib cartilages less on	Line of symphysis oblique, and lower on	Longitudinal curve present and convex to
28	Right	Right	Right	Right	None present
29	„	„	„	Not oblique	„
30	Left (slightly)	Left (slightly)	Left	„	„
31	Right	Right	Right	„	„
32	Left	Left	Left	Left	Right
33 (Page 16)	Symmetrical	Symmetrical	Right	Not oblique	None present
34	„	„	Left	Right	Left
35	Right	Right	Right	Not oblique	Right
36	Left	Left	No difference	„	None present
37	Right	Right	Right	Right	Right
38	Left	Left	Left	Not oblique	None present
39	„	„	No difference	Left	„
40	Right	Right	„	Right	Left
41	Left (slightly)	Left (slightly)	Left (slightly)	Left	„
42	Right	Right	No difference	Right	„
43	Left (slightly)	Left (slightly)	Right	Not observed	Not observed
44	Symmetrical	Symmetrical	Right	„	„
45	Right (slightly)	Right (slightly)	No difference	„	„
46	Right	Right	Right	„	„
47	Left	Left	Left	Left	Right
48	Right	Right	Right	Not oblique	„
49	Symmetrical	Symmetrical	„	Left	Left
50	Right (slightly)	Right (slightly)	„	„	None present
51	Left	Left	Left	Not oblique	„
52	Symmetrical	Symmetrical	No difference	Not oblique	None present
53	Right (slightly)	Right (slightly)	Left	Right	Left
54	Left (slightly)	Left (slightly)	No difference	Left	None present

On analysing the foregoing Table it will be seen that—

(*a.*) Out of 54 sterna a depression of the clavicular facet, accompanied by a diminution of the vertical depth of the manubrium on the same side, was present.

On the right side in 27 (in 3 of which the changes were only slightly marked).

On the left side in 20 (in 6 of which the changes were only slightly marked).

Seven were symmetrical in this regard.

(*b.*) In the same number of sterna (54) the interval between the 1st and 2nd rib cartilages was the same on both sides in 15.

It was less on the right side in 24, on the left side in 15. Of the 24 in which the interval was less on the right side the condition accompanied symmetrical manubria in 3, but in the remaining 21 it was associated with a depressed clavicular facet and a diminution of the vertical height of the manubrium on the same side.

Of the 15 in which the interval was less on the left side, one was associated with a symmetrical manubrium; in the remaining 14 the clavicular facet was depressed and the height of the manubrium diminished on the same side.

(*c.*) Observations were made on the obliquity of the symphysis between the manubrium and gladiolus in 30 sterna only. No obliquity was present in 12; the remaining 18 presented an oblique symphysis.

Of these the symphysis sloped downwards on the right side in 10 (with a depressed clavicular facet, &c., on the same side in 7, on the opposite side in 2, and with a symmetrical manubrium in 1).

The symphysis sloped downwards on the left side in 8 (with a depressed clavicular facet, &c., on the same side

in 6, on the opposite in 1, and with a symmetrical manubrium in 1).

(*d.*) Observations on the longitudinal curvature were made in 46 specimens.

The curve was absent in 19, present in 27. The convexity was towards the right in 15 (with depression of the clavicular facet, &c., on the same side in 10, of the opposite in 5).

The convexity was towards the left in 12 (with depression of the clavicular facet, &c., on the same side in 5, on the opposite side in 5, and with a symmetrical manubrium in 2).

CONCLUSIONS.

The majority of sterna—viz., 87 per cent.—are asymmetrical, while about 13 per cent. are approximately symmetrical.

The most notable irregularity is a depression of the clavicular facet, accompanied by a corresponding diminution of the vertical depth of the sternum on the same side, present on the right side in 50 per cent. of sterna, and on the left side in 37 per cent.

The interval between the articular facets for the 1st and 2nd rib cartilages is usually less on one side than on the other, being narrower on the right side in about 44 per cent., and equal in about 28 per cent. of sterna.

In the majority of sterna the symphysis between the manubrium and gladiolus is oblique, sloping downwards on the right side more frequently than on the left.

Finally there is, in about 59 per cent. of sterna, a longitudinal curve traversing the whole bone, the convexity of which is directed a little more frequently to the right than to the left.

Professor Purser was of opinion that right-handedness depended on the brain. The blood supply to the left brain was more direct, as was shown by the frequency of embolism on that side. In the lower classes no pains were taken to make them right-handed, still there were no more left-handed people among them than among the higher classes.

Professor Birmingham, in reply, referred to the various theories advanced to account for right-handedness. He thought none of them quite satisfactory. He agreed with Professor Purser that the cause lay in the brain.

NOTES ON THE DISSECTION OF A CASE OF DOUBLE KNOCK-KNEE.

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[Read in the Section of Anatomy and Physiology, May 22, 1896.]

ALTHOUGH the condition known as genu valgum or knock-knee is very common, and the literature on the subject is extensive, the opportunities that occur of obtaining specimens of this deformity in a state suitable for careful dissection of the soft parts appear to be very rare, judging from the almost entire absence of such specimens from our museums, and the dearth of any records of their examination. Under these circumstances it is hoped that notes of the dissection of a case of this kind will prove of some interest.

During the Winter Session 1895-6 the body of a female, aged seventeen years, with double knock-knee, was brought to my department. The girl's history, although not so complete as might be wished, showed that the deformity had commenced about two years before her death, and was attributed to an illness of a pulmonary character, which commenced while she was working in a factory. It was therefore evidently a case of genu valgum adolescentium.

The girl was of small size for her age, and very thin. She died from tubercular disease, which involved not only the lungs and pleuræ, but also the peritoneum. There was a cavity in the apex of the right lung, and commencing consolidation at the apex of the left. Both pleuræ

were thickened and adherent, and almost the whole surface of the peritoneum was covered with tubercular nodules.

The degree of knock-knee was very nearly the same in both limbs, and that the deformity was well-marked is evident from the following measurements:—With the knee fully extended, a straight line drawn from the top of the head of the femur to the centre of the ankle-joint passed 6 inches to the outer side of the centre of the knee-joint, instead of, as in a normal limb, corresponding to this point. Again, the external angle at the knee between the long axis of the shaft of the femur and that of the tibia is usually about 170°, while in this case it was 120° in the right limb and 118° in the left. As is usual in well-marked cases of knock-knee, the feet were inverted to an abnormal degree—*i.e.*, in the condition termed talipes varus.

So far as I have been able to ascertain, in none of the many cases of knock-knee that are described is there any reference to the alterations in the positions and relations of the soft parts round about the knee, although on theoretical grounds marked displacement of these structures might reasonably be expected. In order to investigate these points the muscles, blood-vessels and nerves passing over the knee-joint were dissected with as little disturbance of their position as possible. A drawing (Plate, fig. 1) made from the posterior aspect of the right knee shows clearly the marked changes in the position of most of these structures, when compared with a normal limb. Amongst the muscles of this region the sartorius and gracilis were displaced to the greatest extent, as they descended behind instead of on the inner side of the knee. On looking at the knee from behind, the sartorius was seen to lie 1½ inches external to the most prominent point on the inner aspect of the internal tuberosity of the femur, while the gracilis descended behind the knee opposite the outer boundary of the

intercondyloid notch. The semi-membranosus and semi-tendinosus were also displaced outwards, though not nearly to so great an extent as the other two muscles. As a result of the shifting of the sartorius and gracilis from the inner side to the back of the knee, the tendon of the adductor magnus, inserted into the upper part of the internal tuberosity of the femur, was rendered much more prominent and superficial than normal. This is of some importance, as the adductor tendon is one of the guides given by Professor Macewen for the incision in his well-known operation of supra-condyloid osteotomy for the relief of this deformity. The biceps was normal, except that it was rather more prominent than usual, and was at once rendered tense on any attempt being made to adduct the leg at the knee. A reference to the Plate, fig. 1, will show that the insertion of the popliteus and the tibial origin of the soleus were uncovered to a large extent by the gastrocnemius. This is partially due to the feeble development of the latter muscle, but also to the projection inwards of the internal tuberosities of the femur and tibia.

The popliteal artery is sometimes supposed to be specially liable to injury during the operation of division of the femur for the cure of knock-knee; its relations therefore to this bone were carefully examined. The artery pierced the adductor magnus in the usual position, and after a practically straight course terminated as usual at the lower border of the popliteus. Between these two points its relations to the bones were greatly altered. Thus it crossed the popliteal surface of the femur much nearer the outer than the inner border of this area, passed on to the back of the external condyle close to its outer edge, and then descended behind the superior tibio-fibular articulation. As a rule the popliteal artery lies against the back of the upper end of the tibia, about midway

between its internal and external tuberosities, or at most a quarter of an inch internal to that point. In this case it was close to the level of the outer edge of the top of the tibia. The popliteal artery is normally separated, even in thin subjects, by a distinct layer of fat from the popliteal surface of the femur. The distance between the artery and the popliteal surface of the bone was further increased in this case by the vessel lying on the back of the external condyle instead of in the hollow between the two condyles. A finger could readily be passed between the bone and artery without disturbing the latter. For nearly two inches above the external condyle, and opposite the lower part of the popliteal surface, the artery was five-eighths of an inch from the bone. The relations of some of the smaller vessels are also of interest. The deep branch of the anastomotic of the femoral passed down as usual just in front of the tendon of the adductor magnus, but its superficial branch, which accompanies the long saphenous nerve, was displaced on to the back of the knee. The two superior and the azygos articular branches arose by a common trunk, which passed inwards to about the middle of the back of the joint before dividing. The superior external articular continued in the same direction as the common trunk for a short distance, and then turned outwards, passing between the popliteal artery and the femur, to be distributed as usual. At the point where the external articular artery turned outwards it was five-eighths of an inch internal to the popliteal vessel. The external and internal popliteal nerves were shifted outwards to even a greater extent than the popliteal artery. This was especially the case with the internal popliteal, which at its origin from the sciatic was situated in a plane external to that of the outer surface of the lower part of the shaft of the femur. As the nerve

descended it crossed the outer head of the gastrocnemius obliquely from without inwards, and came in contact with the popliteal artery near its termination.

In the descriptions that have been published of specimens of knock-knee attention has been directed almost entirely to the condition of the bones of the affected limb; but even amongst these there are comparatively few records of cases in which the deformity came on about puberty, and the bones were examined before the epiphyses had united with the diaphyses. Indeed, with the exception of Mikulicz' work[a] the literature on this subject is almost nil, although the conditions found in such cases are of great importance in determining the pathological nature of the deformity. In my specimen the upper end of the femur, and almost the whole length of its shaft, appeared to be normal, and showed no unusual curves, the changes in the form of the bone being confined to the neighbourhood of the lower end of the diaphysis and its epiphysis. When the femur was placed with the long axis of its shaft vertical, the internal condyle lay at a plane one inch lower than that of the external, whereas, in a young and normal femur, of almost exactly the same length, I found that the downward projection of the internal condyle was only one-fourth of an inch more than that of the external. It has been frequently asserted that in cases of knock-knee the internal condyle is abnormally increased in length, but Mikulicz has endeavoured to show that this is not the case, the marked prominence of the internal condyle being due to an increased growth of the inner part of the lower end of the femoral diaphysis, so that the epiphysis is placed against a surface looking obliquely downwards and outwards. It is evident that this point can only be determined

[a] Die seitlichen Verkrümmungen am Knie und deren Heilungsmethoden. Langenbeck's Archiv. f. klin. Chirurgie. Bd. XXIII.

with accuracy by the examination of the femur in cases of knock-knee before the epiphysial cartilage has become ossified, and preferably shortly before adult life, when this cartilage is thin, so that the line between diaphysis and epiphysis can be sharply defined. Even, however, in such favourable circumstances the problem is not so simple as might be supposed, for the opposing surfaces of epiphysis and diaphysis are very uneven, both in normal and deformed femora, and the articular surfaces are convex, so that satisfactory points from which to measure the relative length of the two condyles are not readily found. Professor H. E. Clark[a] made a series of observations of normal adult femora to determine this question, and he came to the conclusion that the internal condyle is, as a rule, a quarter of an inch longer than the external. These measurements, however, represent the greatest vertical extent of the epiphysis on the inner and outer sides, and not simply the vertical length of the condyloid articular surfaces, for they were, I believe, made on the inner side from the adductor tubercle, and on the outer from just above and in front of the depression for the outer head of the gastrocnemius. In these two places the epiphysial line attains its highest level above the condyles. On the posterior aspect of the femur the epiphysial line is situated immediately above the articular surface of the external condyle, but is fully an eighth of an inch above that of the internal condyle, and then ascends to the adductor tubercle, which is a quarter of an inch above the level of the top of the articular surface of the internal condyle. The vertical extent of the posterior part of the two condyloid articular surfaces will be found to be practically equal, although the vertical height of the epiphysis is a quarter of an inch more on the inner than the outer

[a] See Macewen's Osteotomy, p. 44.

side of the bone. Surgeons have differed as to whether or not, in cases of knock-knee, the internal condyle is absolutely, and relatively to that of the external, increased in length, so that the points selected for the measurement are of interest. Mikulicz draws one transverse line at the back of the femur just above the posterior part of the two condyles, and another at the level of the lowest part of the two condyles, and he states that these two lines are parallel, or very nearly so; hence he regards the two condyles as of equal length.

In my specimens of knock-knee the two condyles, measured by Clark's method, were found to be only one-eighth of an inch different in height from one another, the internal being $1\frac{1}{8}$th inch and the external one inch, and by Mikulicz' plan equal. Compared with normal bones of the same size there was no increased length of the internal condyle, if anything slightly the reverse, while at the same time there was no shortening of the external. Further, the shaft of the bone was not abnormally curved, so that the excessive downward projection of the internal condyle, when the shaft of the bone was held with its long axis vertical, was due to an increased growth of the inner side of the diaphysis. These results support the views of Mikulicz.

An examination of the tibia showed that it contributed even more than the femur to the production of the in-knee deviation, and that this was due entirely to an excessive growth of the inner side of the upper part of its diaphysis. Thus with the shaft vertical the inner edge of the upper surface of the tibia projected one inch and an eighth above the level of the outer edge, and on coronal section (see Plate, fig. 2), the epiphysis was rather thinner on the inner than the outer side.

In both tibia and femur the layer of compact bone was

thinner than normal, and at the extremities the spaces in the cancellous bone were much larger than usual. This was especially the case towards the inner side of the bones. The epiphysial cartilage at the lower end of the femur presented appearances which are characteristic of rickets. In some places it was considerably thickened, and formed various irregular projections, almost all of them being directed towards the diaphysis. There were also various islands of cartilage embedded in the bone above and below the epiphysial line. The epiphysial cartilage at the upper end of the tibia presented some indications of ricketic changes, but they were not nearly so well marked as in the femur.

There was no deformity in the bones of the upper limb except some swelling of the extremities of the long bones and slight rachitic changes in their epiphysial cartilages. The vertebral column and ribs were normal, but the pelvis was imperfectly developed and deformed. The conjugate diameter of the pelvic inlet was considerably greater than the transverse, and the right innominate bone at the ilio-pectineal eminence was bent inwards.

The spine on the inner side of the upper part of the shaft of the tibia, generally found in cases of knock-knee, was well marked in both legs. Its mode of production is obscure. It was situated half an inch below the level of the epiphysial cartilage, and behind and above the insertions of the sartorius, gracilis and semitendinosus, but the lower part of the internal lateral ligament of the knee was attached to it, and its development is possibly due to the strain upon this structure.

The articular cartilages covering the femur, tibia, and patella at the knee-joint appeared to be quite normal. The trochlear surface of the femur and the corresponding articular aspect of the patella were practically normal.

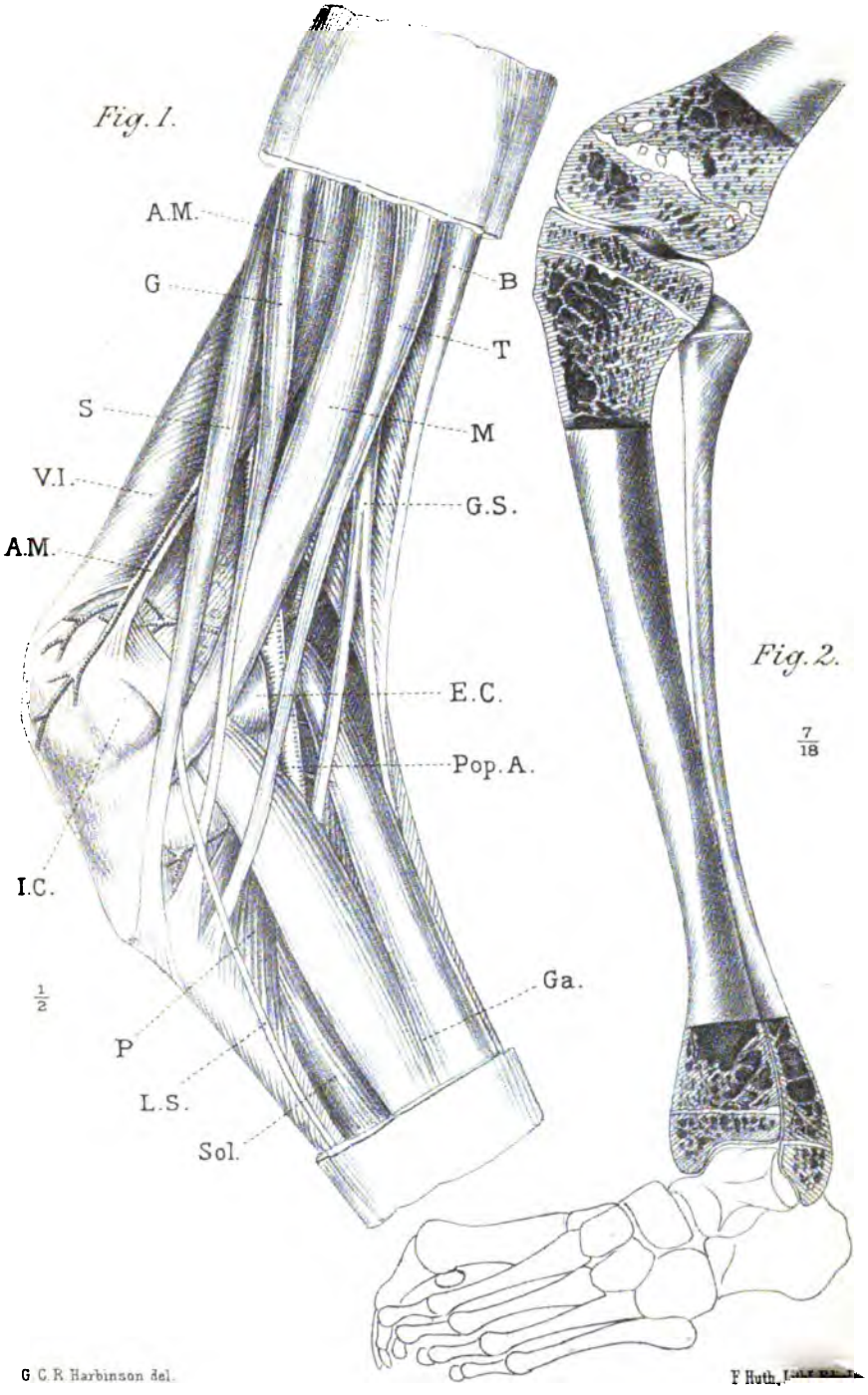
The muscles of the body generally were feeble and the ligaments of the joints lax, but I could find no evidence of any special atrophy of any muscle or group of muscles in the thigh, or any distinct softening or relaxation of the internal lateral ligament of the knee. On attempting to straighten the knee I found that the ilio-tibial band, the biceps, and the external lateral ligament became tense, and resisted the reduction of the deformity.

EXPLANATION OF PLATE.

Fig. 1.—View of the posterior aspect of the right knee ($\frac{1}{2}$); I. C., posterior part of internal condyle of femur; E. C., posterior part of external condyle of femur; A. M., adductor magnus; S., sartorius; G., gracilis; T., semitendinosus; M., semimembranosus; B., biceps; P., popliteus; Sol., soleus; Ga., gastrocnemius; G. S., great sciatic nerve; Pop. A., popliteal artery; L. S., long saphenous nerve.

Fig. 2.—View from the front of the lower end of the femur, the tibia and fibula, and the foot, of the left lower limb, from a case of double knock-knee ($\frac{7}{12}$). At the knee the femur and tibia were cut in a coronal direction to show the relation of epiphyses and diaphyses, and the appearance of the epiphysial cartilages. The lower ends of the tibia and fibula were divided in a similar manner.

PROFESSOR PURSER said that judging merely by the naked-eye appearances, one half of the epiphysial cartilage appeared rachitic, and the other half normal.



DR SYMINGTON ON GENU VALGUM.

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